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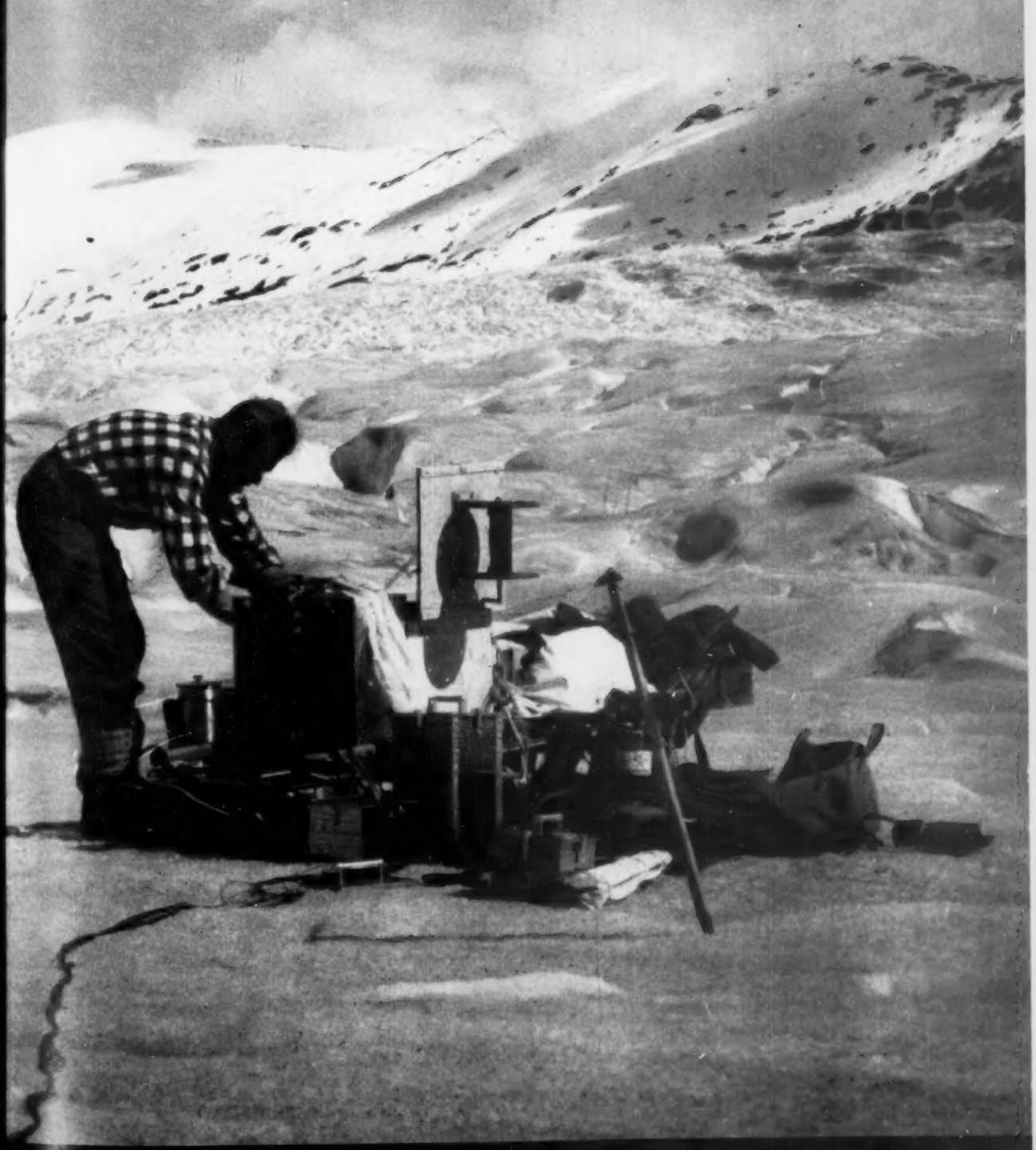

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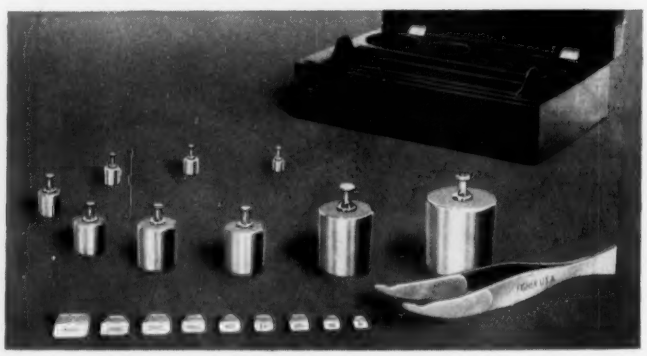

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(Photo courtesy Stanford Research Institute)

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Publication office, Business Press, Inc., 10 McGovern Ave., Lancaster, Pa. Orders for subscriptions and requests for change of address should be directed to the Circulation Department, A.A.A.S., 10 McGovern Ave., Lancaster, Pa., or 1515 Massachusetts Ave., N.W., Washington 5, D. C. Subscriptions: $7.50 per year; single copies 75 cents. Four weeks are required to effect changes of address.

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THE
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JUNE 1950

The Electrophoretic Detection of Plant Virus Proteins

SAM G. WILDMAN and JAMES BONNER

The senior author took his Ph.D at the University of Michigan in 1942, and from there went to the Division of Rubber Plant Investigations of the USDA. In 1944 he became senior fellow in research, Biology Division, California Institute of Technology. Dr. Bonner (Ph.D., Cal Tech, 1934) is a plant physiologist who has been professor of biology at Cal Tech since 1946. Their article is based on an address given before the Botanical Society of America during the 1949 Meeting of the AAAS, and the work it describes was supported in part by the Herman Frasch Foundation for Agricultural Research and in part by the National Foundation for Infantile Paralysis. Miss Jean Campbell supplied the electrophoresis analyses.

IN PARTICULAR instances the presence of virus in plant tissue may be detected by electrophoretic analysis of the extracted cytoplasmic proteins. The virus protein appears as a new cytoplasmic protein component not present in the tissues of healthy plants. The electrophoretic method is sufficiently sensitive to detect even those viruses present in relatively small amounts in the plant and also to demonstrate the presence of virus protein before the onset of visible disease symptoms.

The present work is part of a general program addressed to the problem of the manner in which viruses reproduce in living tissues. The plant virus is typically a ribose nucleoprotein of high molecular weight, and certain of the plant viruses have been isolated from the infected tissue in the crystalline state. When a plant is infected with a small amount of this crystalline virus nucleoprotein, more virus is produced and relatively much larger amounts may be recovered than were originally used in the inoculation. How is this new synthesis of foreign protein achieved in the living cells of the plant? Are there any related proteins normally present in the plant which may serve as substrate, providing the building blocks for virus reproduction? It is evident that in order to study these questions it is necessary to have information concerning the proteins normally present in the plant and in particular in the leaf, which is the site of many of the virus diseases.

Normal leaf proteins. Methods for the large-scale preparation and study of the normal leaf proteins have been developed over the past five years and have been in part already reported. Since the relatively thin protoplasmic layer of the plant cell is enclosed by a tough cell wall, it is essential to first grind the tissue in such a manner as to rupture the cell walls and liberate the protoplasmic contents of the cells. The broken cell walls are then caught on a porous filter paper. The resultant green suspension, or whole protoplasm, consists of a

suspension of the particulate matter of the protoplasm, of which the chloroplasts are the conspicuous material, together with fragments of nuclei suspended in a solution of the soluble cytoplasmic proteins and vacuolar components. Various methods may be used to free the particulate matter from the cytoplasmic fraction, and that given in Figure 1 has proved satisfactory in virus work. The green suspension is frozen, thawed, and centrifuged at 5,000 times gravity. The particulate matter is removed as a green precipitate, leaving behind a clear brown supernatant, the whole cytoplasm. The latter fraction contains not only the soluble cytoplasmic proteins but also the soluble low molecular weight constituents of the plant, including sugars, amino acids, plant acids, etc., from which the proteins may be separated by dialysis. The next question to concern us, then, is how many proteins and what kinds make up the cytoplasmic complement of the plant cell.

The species of proteins in a mixture may be readily separated from one another by the use of the electrophoretic method worked out in general outline by Tiselius. A concentrated sample of the protein mixture is placed in the center of a suitable vessel or cuvette so that the solution of protein in buffer is separated by sharp interfaces from the adjoining regions of buffer alone. An electric current is then passed through the solution. Since protein molecules contain a net electrostatic charge (except at their isoelectric points), a portion of the current will be carried by them and each protein in the mixture will migrate through the cuvette as a moving front, or boundary. The distance which this boundary travels in unit time is a function of the charge on the protein molecule, and, since each species of protein molecule will in general possess its own characteristic charge, different protein species will migrate at different rates and will maintain their own characteristic moving boundaries. By the use of a suitable optical system it is possible to detect and plot these moving protein boundaries, a process which yields the scanning patterns shown in the lower portion of Figure 1. The distance which the protein has traveled is measured by the distance along the abscissa. Each protein boundary is responsible for a single peak in the scanning pattern, the area under the curve which makes up the peak being a direct measure of the amount of that protein present in the solution.

The pattern at the lower left of Figure 1 shows the results obtained on the electrophoresis of the whole cytoplasmic protein of normal, healthy tobacco leaves. One principal component makes up

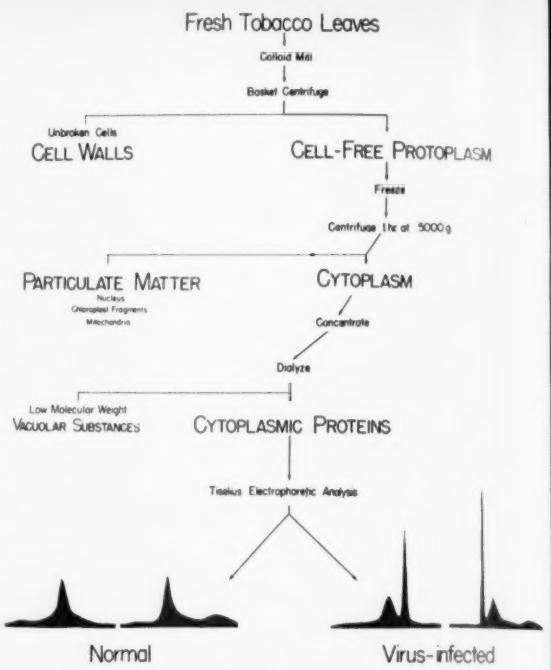

FIG. 1. Methods used in the preparation of cytoplasmic protein for examination by electrophoretic analysis. Scanning pattern of the whole cytoplasmic protein of normal healthy tobacco leaves is shown at the lower left (descending and ascending boundaries). The sharp peak is that due to normal cytoplasmic nucleoprotein. In the lower right is shown the scanning pattern of whole cytoplasmic protein of leaves infected with tobacco mosaic virus. The sharp new spike is that due to virus.

85 per cent or more of the total cytoplasmic protein, and the remaining 15 per cent consists of a number of minor proteins. In the example of tobacco leaves these minor components move more slowly than does the major component, and their peaks blend together to form a continuum. These minor proteins can, however, be themselves resolved to a considerable extent after chemical separation from the main component, by the use of suitable conditions.

The major component of tobacco leaf cytoplasm appears to be, like virus, a pentose nucleoprotein. It possesses in addition interesting enzymatic properties which need not concern us here. This normal nucleoprotein may be separated from the accompanying minor proteins by fractional precipitation with 0.38 saturated ammonium sulfate to yield a protein which appears to be homogeneous both in the electrophoresis apparatus and in the ultracentrifuge.

The leaves of species other than tobacco which have been investigated yield cytoplasmic proteins separable by the same methods. In all the cases that have been studied to date, the cytoplasmic proteins contain a main nucleoprotein component of

generally similar chemical and physical behavior. In all cases, also, the remainder consists of a mixture of small amounts of several or many other components.

Tobacco mosaic virus. Let us now ask ouselves what happens to the protein spectrum of the plant after infection with a virus. Tobacco mosaic (TMV) was chosen for these first experiments since it is produced in infected plants (tobacco) in relatively large amounts and there already exists a large body of information concerning the physical and chemical character of the virus. Thus, a short while after infection, 25 per cent or more of the total plant protein can be accounted for and can be isolated as the highly infectious new virus protein. For the present experiments, tobacco plants were inoculated with TMV in an apical leaf and the virus allowed to spread through the plant spontaneously. The lower leaves were then harvested after twelve days, the whole cytoplasmic proteins prepared by the methods already described, and the mixture subjected to electrophoresis. The results, shown in Figure 1 (lower right), are clear. A new cytoplasmic protein component has appeared in addition to those present in the cytoplasm of the healthy plant. This new component yields a scanning pattern entirely different from those of the normal proteins, namely, a tall, narrow spike such as is characteristic of very high molecular weight proteins. This is in accord with the supposition that the component is virus, since the virus protein possesses a molecular weight in the neighborhood of 40,000,000, whereas the normal cytoplasmic nucleoprotein, for example, has a molecular weight of the order of 500,000. That the new component is indeed virus was shown by isolation of the material by chemical and physical methods and demonstration of its high infectivity upon inoculation into healthy plants.

The course of appearance of TMV in infected tobacco plants may be readily followed by the electrophoretic method, as is shown in Figure 2, which gives scanning patterns of whole cytoplasmic protein of tobacco leaves harvested at various times after infection of the plants with virus. Figure 2 shows that within three days after infection a small but definite new peak, that characteristic of TMV, appears in the scanning pattern of the extracted cytoplasmic protein. This new component becomes progressively more abundant until it attains a relatively stable level after twelve to sixteen days. It is of interest that the visible symptoms of the disease do not develop until roughly two weeks after infection, so that the presence and increase of the virus protein in the leaf can hence be detected electrophoretically before they may be detected by visual study of the plant.

Figure 2 demonstrates a further significant fact, namely, the inverse relationship between concentrations of virus and normal nucleoprotein among the cytoplasmic proteins of the leaf. As the new virus protein appears and increases in amount, the normal nucleoprotein decreases to a corresponding extent. It has been shown in work which has been separately reported that the other proteins of the leaf are not similarly affected during the rapid production of TMV, and that the reciprocal relation seems to be confined to the normal, cytoplasmic nucleoprotein alone. It is additionally known that during these early stages of virus development, the amount of total protein of the leaf remains unchanged. These facts suggest that the TMV protein is somehow produced at the expense of the normal cytoplasmic nucleoprotein. Comparative analytical

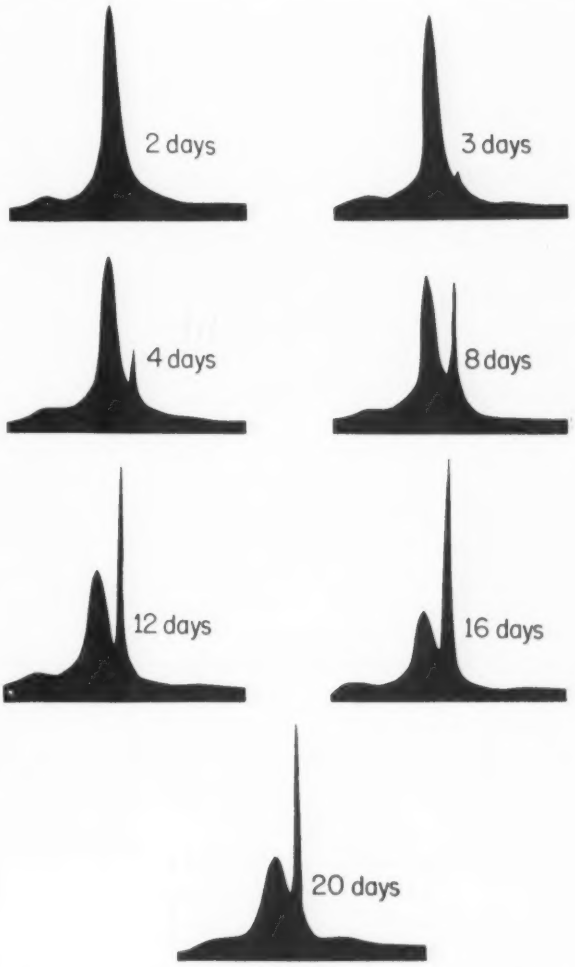

FIG. 2. Electrophoretic scanning patterns of whole tobacco leaf cytoplasm prepared at various intervals after infection with tobacco mosaic virus.

data on the compositions of TMV and the normal nucleoprotein are also in accordance with this view, since the two materials are remarkably similar in their content of phosphorus, purine, and certain of the amino acids. The close relationship between TMV protein and the normal cytoplasmic protein is further emphasized by the fact that antibodies prepared to cytoplasmic protein cross-react strongly with pure TMV, and vice versa. Thus there are features in the pattern of the normal nucleoprotein which it shares with the TMV molecule. Whether the virus is made directly from the normal protein or whether, as seems more likely, the virus is in part made from the normal nucleoprotein and in part from other components, remains to be determined.

Other sap-transmitted viruses. The fact that the presence of TMV in tobacco leaves may be so readily detected by electrophoretic methods suggests at once the possibility that the same method may be applicable to other viruses. Experiments were therefore done with two other viruses which, like TMV, are transmitted only by mechanical means. In these instances infection was accomplished merely by rubbing the sap of infected leaves over the surface of the leaves of healthy plants. Figure 3 compares the electrophoretic scanning pattern of the whole cytoplasmic protein of healthy cucumber leaves with that of cucumber leaves infected with cucumber mosaic virus. It may be seen that the cytoplasm of the infected leaves differs from that of the normal by the presence of a distinct new component, in this case one which migrates less rapidly than does the normal cytoplasmic nucleoprotein.

The water cress virus of Mr. C. C. Cheo may be

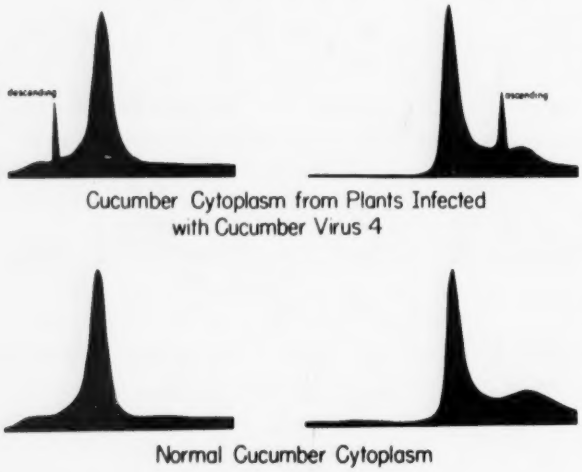

FIG. 3. Electrophoretic scanning patterns of whole cucumber leaf cytoplasm prepared from healthy cucumber leaves and from leaves of plants infected with cucumber mosaic virus.

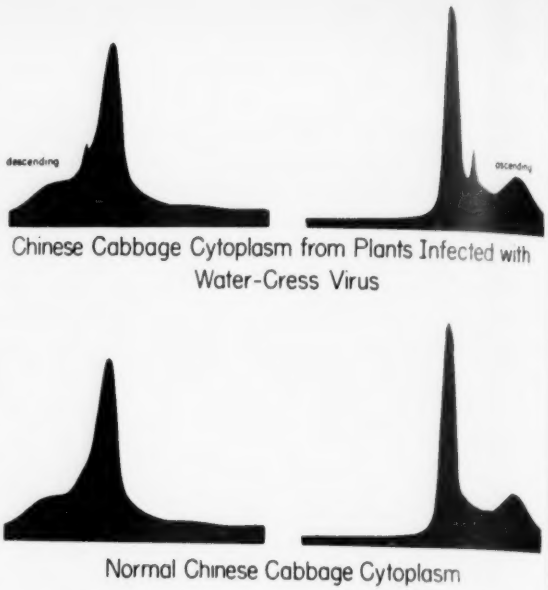

FIG. 4. Electrophoretic scanning patterns of whole Chinese cabbage leaf cytoplasm prepared from healthy Chinese cabbage leaves and from leaves of plants infected with water cress virus.

similarly detected electrophoretically among the cytoplasmic proteins of infected leaves of Chinese cabbage, as is shown in Figure 4. The new protein appears in amounts which are small in comparison to those experienced with TMV.

We have, then, three different sap-transmitted virus diseases in which the presence of virus protein among the cytoplasmic constituents may be detected by electrophoretic means, a relatively simple procedure. The presence of the new virus-induced component may further be detected in infected plants before the appearance of visible disease symptoms.

The case of curly top, an insect-transmitted virus. Curly top, a serious disease of sugar beets, is, like many virus diseases of plants, transmitted only by a specific insect vector—in this case the leafhopper—and never by contact transmission. The disease does, however, move readily within the plant and may in fact be transmitted from one plant to another through a graft union. It has not yet been possible to isolate the curly-top virus from the plant, and the virus has not been characterized chemically. A comparison of the electrophoretic scanning patterns (Fig. 5) of healthy and curly-top-infected leaves of tobacco (an alternative host for the virus) reveals that the normal protein spectrum of cytoplasm has been markedly altered as the result of virus infection, but it has not yet been feasible to demonstrate unequivocally, as with TMV and other plant

Normal Turkish Tobacco Cytoplasm

Turkish Tobacco Cytoplasm from Plants Infected with Curly-Top Virus

FIG. 5. Electrophoretic scanning patterns of whole tobacco leaf cytoplasm prepared from healthy tobacco leaves and from leaves infected with the insect-transmitted curly-top virus.

Normal Citrus Cytoplasm

Citrus Cytoplasm from Trees Infected with Psorosis Virus

FIG. 6. Electrophoretic scanning patterns of whole leaf cytoplasm prepared from healthy citrus leaves and from leaves of trees infected with the graft-transmitted psorosis virus.

viruses, whether a new protein is present in infected cytoplasm. The present results do justify the hope, however, that it may be possible ultimately to detect the presence of insect-transmitted viruses in the plant by electrophoretic analysis and to separate the new component from other leaf constituents, using the electrophoretic mobility of the protein as a virus character, rather than having to depend entirely upon the laborious and difficult insect-transmission test.

Viruses transmitted only by grafting. It has been noted above that viruses in general move about readily within the plant and may be transmitted from one plant to another across a graft union. For this reason graft transmission is used as a criterion in the establishment of a disease as due to a viral agent. There are, indeed, certain diseases which are so far known to spread only by the grafting of diseased or infected tissue to healthy plants, and never by insects or contact. Such a virus disease is psorosis, an affliction of citrus. This disease is spread by the use of infected budwood in the grafting of nursery stock, although the symptoms of the disease may not appear in the trees for months or years after infection. The virus has not been isolated. The leaves of psorosis-infected citrus trees nonetheless yield a clearly distinguishable new protein component not present in the leaves of comparable healthy trees, as is shown in Figure 6. It is of interest to note that this new component is present among the cytoplasmic proteins of infected trees, even though the leaves show no obvious symptoms of the disease, and will succumb to it only after a period of several months or years. It may be concluded, therefore, that electrophoretic analysis may be of utility in the detection and isolation of viruses in the exclusively graft-transmitted group also.

These experiments show that in several instances the presence of a virus in a plant tissue is correlated with the appearance of a new and characteristic protein component in the electrophoretic scanning pattern of the cytoplasmic protein of the tissue. In one instance, that of TMV, this new component has been isolated and shown to be the virus itself. Whether or not the new component is virus in all cases, the association of the new protein with the presence of virus disease establishes the possibility of empirical diagnosis of the presence of virus by electrophoretic examination of the cytoplasmic proteins. The electrophoretic mobility of the new component may further prove to be a useful character in the isolation and purification of the new protein component which arises in plant tissues as the result of virus infection.

Science and the Literary Mind

CHARLES I. GLICKSBERG

Dr. Glicksberg (Ph.D., Pennsylvania) was born in Warsaw, Poland, but grew up in New York City. Originally he intended to be a physician, but his interest in literature led him to abandon this idea, and he is now in the Department of English at Brooklyn College. Previous articles of his appeared in THE SCIENTIFIC MONTHLY *in July and December 1944, May 1946, and January 1947.*

WHAT irks the literary man is never science in the concrete but always science in the abstract. He does not attack any specific conclusions of physics or biology, astronomy or anthropology, but invariably Science personified and malign, Science the Great Destroyer, Science the Quantitative, the Enslaver of the Human Personality, the Breeder of Fascism. (It would indeed be interesting if someone were to compile an anthology of antiscientific abuse drawn from the writings of twentieth-century literati.) He knows enough not to make a frontal assault on the citadel of the laboratory. The facts are as given; he dare not, and cannot, dispute the nature of the evidence.

What arouses so much animus in the literary mind is the philosophy of science, its view of the universe. In a world stripped of divine authority, there is no governing principle of absolute justice, or mercy, or lovingkindness, no promise of heaven and no threat of hell. Man is orphaned in the universe, and Energy, not the human personality, is the central protagonist on the cosmic stage of Space-and-Time. The writer finds such a universe of discourse not only "undramatic" but insufferable. It robs him of faith in the freedom of will and the unique importance of creative genius. To escape from the iron trap of necessity he must, like Gide, resort to the gratuitous act, he must indulge in the irrational for its own sake.

It is a relatively easy matter to cite copious evidence of the aversion of the literary mind (there are, to be sure, notable exceptions) to the scientific outlook. One could turn to such prophets of dissent as T. S. Eliot, Lewis Mumford, Waldo Frank, Allen Tate, John Crowe Ransom, Jacques Maritain, Aldous Huxley. Perhaps the most inspired and influential spokesman of the movement was D. H. Lawrence, who worked with the lightning rod of intuition. He constructed a science, a subjective science, based on vital intuitions. We glean some revealing hints as to the nature of this subjective science when we learn that it is not concerned with cause-and-effect phenomena. Science, Lawrence declared, may be perfect within its limited sphere, but it is absurd to assume that it exhausts the whole scope of human possibility in knowledge—as if science ever had laid claim to such powers. Determined to rid the world of the incubus of reason and to dispose once and for all of a science that is objective, mechanical, and therefore dead, he fell back upon the most curiously distorted "reasons," a solar-plexus kind of logic, to drive home his thesis. Prophetically he announced that he did not believe in evolution, "but in the strangeness and rainbow-change of ever-renewed creative civilizations." Thus what we are given is the religion of the creative artist, a religion bound to the earth and the instincts. Lawrence went further to argue that the primal consciousness of man is premental, that mind is but the last flower. Indeed, effective consciousness, he assured us, is always nonmental. The individual is the supreme fact, not to be annulled by the arbitrary edicts of science. The theory of relativity only serves to deepen this precious sense of individuality amidst the vertiginous flux. The universe has, alas, been reduced to a mathematical symbol. What can science give us, after all, but a confusing abstract formula, destroying the essence of things?

Few of the literati, however, have been as honest, explicit, and thoroughgoing as Lawrence in making clear the reasons for their repudiation of science. For, whether they have liked it or not, they have been strongly influenced by the scientific method of thought, and their faith, when they take the trouble to formulate it, at least wears an austere mask of rationalism. God is no longer in his heaven. All is wrong with the world. We no longer need theological hypotheses in our thinking in order to account for the world we live in. Energy is our God, and Einstein is its articulate prophet. The anthropocentric illusion, though it still lingers regressively in some literary minds, has been abandoned for good. Whether he enjoys the prospect

or not, man is an integral part of nature, and only in his own eyes is he a special manifestation of cosmic energy.

It is precisely this which is anathema to the stubbornly conservative literary mind: the renunciation of purpose, the introduction of seeming lawlessness into the universe, the reduction of the unique human personality to utter insignificance. The ultimate cause or meaning of the life force—we know it not; and even if we could isolate it we are aware that it is neither "good" nor "evil," not to be apprehended by categories of value. It is not only neutral but unconcerned, blind and deaf to human desires. This is a difficult, terribly unpleasant doctrine to swallow. As Carl L. Becker pointed out in *The Heavenly City of the Eighteenth-Century Philosophers*: "It has taken eight centuries to replace the conception of existence as divinely composed and purposeful drama by the conception of existence as a blindly running flux of disintegrating energy." And in some literary minds it has not yet been replaced.

But science marches on inexorably. To those who are still convinced that science is a narrow field of specialization which exerts comparatively little influence on the important cultural forces of our time, it is recommended that they study the basic assumptions underlying English and American poetry of the eighteenth and nineteenth centuries and compare them with those which enter into the poetry of our own time. The poets of the past two centuries, though they wrestled earnestly with serious ontological problems, had no doubt that the universe was filled with a purpose, even if it was inscrutable, and that it was harmoniously designed and governed. What was more, their anthropocentric vision was evident in their rooted faith that this providential cosmic order entertained conspicuously benevolent intentions toward mankind. This view included or implied a belief in the existence of God. As Joseph Warren Beach has made clear in *The Concept of Nature in Nineteenth-Century English Poetry* (1936), even those writers who were not orthodox in their professions of Christianity tended to accept this view of the world. That is to say, one could reject supernatural or revealed religion and still cling to a religious principle—God as a force immanent in the universe.

Even the course of religious thought was profoundly affected, as it was bound to be, by the development of science. For science contributed to the idea of universal laws, and this, combined with the notion of purpose, supplied the groundwork of a teleological design. Thus modernized religion, free from the shackles of formal theology, was made reasonable, but it could still nourish man's craving for an intelligible and fundamentally moral universe. In twentieth-century poetry, however, as the concept of purposiveness and benevolence was destroyed, the religious interpretation of nature (witness the writing of Robinson Jeffers) virtually disappeared. Thus at one blow this tremendously consoling interpretation of nature was abandoned. As Joseph Warren Beach has declared: "And we can have no more striking evidence of the critical force of the scientific spirit in modern times than that it should so soon have sapped and broken down the structure of poetic thought which it did so much to build up." There is, however, really nothing to wonder at in this process. Poetry was bound to change radically with the spread of science.

I

There is sufficient evidence, as Marjorie Hope Nicholson made clear in *Newton Demands the Muse* (1946), that poets in the past were influenced in their thinking and their observation of nature by the work of scientists. Newton's influence upon poetry persisted until the middle of the eighteenth century, and it was only by the time the romantic reaction set in that he was discredited as representing all that was analytical, geometrical, and antithetical to the spirit of poetry. To the imagination of the romantic poets Newton was the archenemy. Yet Wordsworth had admired Newton, and Shelley in particular was excited by the impact of new scientific theories, which became a constituent part of his mind. It was only Keats who protested that the loveliness of earth had perished when scientists stripped nature of her beauty.

The scientific outlook now suffuses our whole mentality. It has altered, as Whitehead indicated, not only our metaphysical presuppositions, but also the imaginative contents of our minds, with the result, unfortunately, that modern man has become a divided being. Mechanical determinism is at war with his fundamental and enduring faith in progress, in self-determination, in the high destiny of mankind. But science has destroyed the old, substantial pillars of faith. In his elegy *In Memoriam*, Tennyson could write:

> Strong Son of God, Immortal Love,
> Whom we, that have not seen Thy face,
> By faith, and faith alone, embrace,
> Believing where we cannot prove.

Very few, if any, of the seventeenth- and eighteenth-century poets could have penned these lines,

for they express a state of mind that would have been completely foreign to their thinking, their view of the world: the intrusion of doubt, the realization that the existence of a supernatural being could not be empirically proved and that he must therefore be taken on faith, if taken at all.

The degree to which the poet is influenced by science is, of course, not explicitly rendered in his work, since he does not communicate bare ideas but emotionalized experience. Thomas Hardy's poetry, for example, is instinct with a pessimistic disillusionment born of scientific determinism. Hardy does not call it that, since he has no need for scientific terminology. It would, in fact, defeat his creative purpose, which is, in a poem like "New Year's Eve," to convey his sense of the desolate meaninglessness of life, the utter futility of time's progression. Though another year has passed, what (the poet inquires) is the good of it all? Accusingly he asks God:

> What reasons made you call
> From formless void this earth we tread,
> When nine-and-ninety can be read
> When nought should be at all?

This is what might be called a leading question, inadmissible in scientific discourse, but perfectly legitimate in the realm of poetry. Though he conceives of the force that rules the universe as devoid of consciousness, Hardy has no hesitation in making him, or it, reply according to form. God simply confesses that whatever he wrought was without any suspicion that he had given birth to Consciousness.

The battle still goes on, and will continue to be waged with increasing intensity. Some contemporary poets, hearkening to the recent outcry against the fruits of science, would keep poetry uncontaminated by the scientific plague. Even a poet like Karl Shapiro, whose quarrel is not with science proper but with the confusion it has wrought in the language of poetry, has this to say in "Essay on Rime":

> The broad use of the raw, untreated data
> Of science and of whole experience
> Expresses a more serious confusion
> In rime than the chaos of prosody.

The complaint, in short, is that science has destroyed not only the sense of mystery but also the power of belief. The age of faith is dead.

There is, as a matter of sober fact, not much substance to these charges, for science is not a generator of belief or value; it merely sets the conditions under which valid beliefs about the universe and man's place in it can be held. Empirically it studies the facts of the actual world and devises hypotheses, theories, "laws," to interpret the facts thus brought forth. The hypotheses and the factual observation interact, one verifying and fructifying the other. Facts in themselves are neutral and meaningless. Theories without empirical substantiation are irrelevant, and such speculations must rightly be rejected as "metaphysical." It is the scientific method which offers a self-regulating process of testing the canons by which judgments are formed and accepted as valid. Science does not provide prescriptive dogmas nor formulas of thought nor even the bare material of poetry. It merely confines the poet when he ventures beyond the realm of direct, personal experience and indulges in lofty philosophical generalizations, proffering his views on human nature and his universal conclusions on life. Science exercises no monopoly and has no private pipe line to "truth." It does not even maintain that it deals with all aspects of experience.

II

What we have been trying to show is that the modern mind has been, for better or for worse, decisively influenced by the growth of science. What is novel about our age is the extent to which the scientific outlook has been diffused. Not that science is without its own presuppositions. The point is simply that for the modern mind these presuppositions are empirically more reliable than the ideological assumptions of the past. For us, whether we like it or not, the laws of physics are the laws of fate. Not that science insists upon unconditional conformity to its views. There is always the possibility of error. If Whitehead is right, and the conception of scientific materialism is unsuited to our present situation, then we shall have to undertake the difficult work of revision. But if it is to be accepted as valid, whatever revision we introduce into the scientific interpretation of the universe will have to be worked out and tested by scientific methods, not by intuition or literary fiat. Until we are convinced of the fact that our present picture of the world is incomplete, we must base our conclusions on the faith that the universe is not held together in an arbitrary fashion, that mystery is not the final word. Whitehead may be justified in his attack on the philosophy of mechanism which was inherited from the seventeenth century and later taken for granted. It is perfectly true that mechanical materialism has landed Western thought in an untenable contradiction. On the one hand, we affirm the ultimacy of matter and, on the other, we declare our belief in the world of man as composed of self-determined organisms.

All one can reply is that science does not presume to solve all the riddles of the universe. There is still much to be discovered. Science simply goes ahead with its patient task of exploring the world of nature on the principle that empirical knowledge, however incomplete, is better than a mystical hunch.

In any event, it is clear that the literary mind will not escape either horn of the dilemma by simply ignoring the scientific outlook. If mechanistic materialism is valid, then what is the alleged freedom of the will but a consolatory fiction? If the universe is composed of molecules that blindly run, then the body, which is also composed of molecules, is not exempt from the operation of natural, universal law. Hence the conclusion that one is not responsible for the actions of the body, and what is the mind but an organic part of the body? If that seems to sweep away all sense of moral responsibility, such a disturbing *non sequitur* can be supported by reference to the Freudian theory of instincts and the psychoanalytic doctrine of the primordial unconscious. In short, the writer, whether he accepts or rejects, must take a stand on these issues. The most formidable objection of the poets and literary critics against science is that it is too completely absorbed in a world of abstractions, thus destroying the immediate, sensory, qualitative value of objects of perception, the facts and phenomena of nature as our senses apprehend them. The concepts of science, valid within their framework, must be supplemented and enriched by the aesthetic intuition of the poets. Certainly the world of culture would be incalculably poorer if we had to do without the exquisite and profoundly beautiful contribution of the poets. But it is one thing to recognize this limitation, which science frankly acknowledges, and quite another to war intemperately and often ignorantly on the alleged aberrations of science.

Consider the extensive influence that a philosopher like Bergson has exerted on modern letters. A mystic, a defender of religion, and an acute critic of the life of reason, he used intuition to disparage the intellect and depreciate science. What he wanted to reinstate was not only indeterminism but freedom, and he gave the modern literary mind the metaphysical rationalization it needed in order to repudiate the fruits of science. Around his banner of intuition rallied all those who found cognition and rational thinking abhorrent, a brake on their creative instinct, a crippling limitation on the variety of totality of experience.

Literature revolted against the hegemony of science by lustily exploiting the weapons that science had put into its hand. When the causal principle was impugned, the fiction of naturalism suffered a temporary decline. Here was scientific evidence to demonstrate that the energy of the universe could not be explained on a rigorously simple deterministic basis. Erratic and unpredictable, the behavior of the atom could be accounted for only on the ground of statistical probability. Thus the mechanistic conception of the universe and of man collapsed of its own weight. The universe could now be legitimately interpreted as the drama of a mathematical god: matter was nothing less than energy. Space and time were bound together in a union which no analytic thought could hope to break asunder. The space-time continuum supplanted the archaic world of Newton, Darwin, and Herbert Spencer. The literati felt liberated by this surprising projection of a universe fundamentally inexplicable according to old theories and laws. At last the life of intuition stood vindicated, and the literary war against Zola and his latter-day disciples seemed won. Who could now say with certainty what was real and what was unreal and what stupendous potentialities of emergence, transcendence, and fulfillment were contained in the world of electrons, with "free" wills of their own? Even consciousness was no longer a simple, rational, logical phenomenon but full of fortuitous leaps, as unpredictable as the behavior of electrons. The physics of discontinuity had effected a spiritual revolution in the literary realm.

But the literary men were, alas, premature in celebrating their glorious victory. Temporarily they had succeeded in reaffirming that art and science are incommensurable, but nature and the human intellect are not incommensurable. The nature which the human mind projects by means of its scientific theoretical constructions has little in common, it is true, with the nature which we "know" through our senses. What theoretical science brings to birth is not empirical nature but a system of scientifically validated "fictions." This system is the work of man and can be brought under repeated critical scrutiny. The world, never complete, is in a state of eternal becoming. Science, like poetry, offers perspectives from which the world of phenomena may be viewed. It cannot be *proved* that there is nothing beyond the physical, since such a postulate is beyond the realm of empirical verification, but it can be shown that such "metaphysical" premises are neither efficacious nor fruitful in practice, and that is all we have to do in the case of much "literarious" nonsense on the subject of science.

In this, as in many other matters, Henry Adams

anticipated the direction of future thought. In his old age he grappled with the mathematical formulas Willard Gibbs had drawn up and sought to apply them to the field of history. The unknowable must be fathomed, but he was well aware that the literary and metaphysical methods of the past had signally demonstrated their impotence. Thus he was led to the conclusion that the future of thought and of history lay in the hands of the physicists. However, the metaphysical terror that gripped man as he contemplated the immensity of the physical universe could not be dissipated by educating people, as Henry Adams proposed, in the fundamentals of mathematical physics. For, if the scientific picture of the universe is valid, then, as Pascal long ago realized, man is lost. In *The Mysterious Universe* (1930), Sir James Jeans described why modern man finds the new universe so terrifying:

> We find the universe terrifying because of its vast meaningless distances, terrifying because of its inconceivably long vistas of time which dwarf human history to the twinkling of an eye, terrifying because of our extreme loneliness, and because of the material insignificance of our home in space—a millionth part of a grain of sand out of all the sea-sand in the world. But above all else, we find the universe terrifying because it appears to be indifferent to life like our own; emotion, ambition, and achievement, art and religion all seem equally foreign to its plan. Perhaps we ought to say it appears to be actively hostile to life like our own.

It was this which the sensitive literary mind could not bear and frantically sought by all possible means to repudiate. The literati, despite all their furious jeremiads and mystical paroxysms, have lost the war against science. The naturalistic temper, the scientific method of solving problems, the critical method of inquiry, are here to stay. And this protracted, costly warfare is all so needless. Why should poetry be brought into competition with science? There is no reason, however, why the poet's vision of the world should not be colored by scientific ideas and insights, though he does not copy the language or imitate the methods of science. Why must the vision of the world which he reports aesthetically conflict with the truths of science? There is no more lamentable misconception than the one which argues that if the poet accepts the scientific outlook he is thereby limited to the general and the abstract. The scientist does not deny the psychological reality of dreams, aspirations, desires, and ideals, even though their reality is subjective. All he maintains as a psychologist is that this area of experience can also be brought under scientific examination. Nor does he deny that the object of perception is concrete and sensory as well as a mass of dancing electrons. Hart Crane, for instance, maintained that there was no truth in the belief that science was inimical to the life of poetry. In his book *Art and Scientific Thought,* Martin Johnson has asserted that likeness—not contrast only—characterizes the relation between the scientific and the artistic attitudes. He is hopeful that a future generation will succeed in correlating its science, art, religion, and philosophy, all of which, according to him, are rooted in the need of man to symbolize his experience.

Another attempt at compromise was made by I. A. Richards, a gifted psychological critic. In *Science and Poetry* (1926) he pointed out that science "tells us and can tell us nothing about the nature of things in any *ultimate* sense." But he was honest enough to recognize that science, especially the science of psychology, is only beginning its gigantic task, and that in the course of time our whole scaffolding of traditional beliefs may have to be reconstructed:

> It is very probable that the Hindenburg Line to which the defence of our traditions retired as a result of the onslaughts of the last century will be blown up in the near future. If this should happen a mental chaos such as man has never experienced may be expected. We shall then be thrown back, as Matthew Arnold foresaw, upon poetry. It is capable of saving us; it is a perfectly possible means of overcoming chaos.

Nevertheless, Richards, in this prophetic affirmation of faith, failed to indicate by what means poetry, as opposed to science, might save mankind and overcome chaos. Could it accomplish this by the method Yeats employed, namely, that of making his poetry "pure," building up his own intricate systems of symbols, his own complex, esoteric mythology? Hardy, on the contrary, voiced a metaphysic that was shot through with a scientific pessimism that was, for him, the logical outcome of Darwinism. Jeffers, for his part, projects a poetic universe that is diabolically evil, nightmarish, demented, destructive, peopled by psychopaths.

And there are some writers who can rise above teleological thinking, with its personal stress, its anthropomorphic emphasis. As John Steinbeck and Edward F. Ricketts pointed out in *The Sea of Cortez* (1941): "Non-teleological thinking concerns itself primarily not with what should be, or could be, or might be, but rather with what actually 'is'—attempting at most to answer the already difficult questions *what* or *how*, instead of *why*." That is well put. Teleological thinking leads people to accept answers as final solutions and tends to force the universe into a Procrustean frame. Not that nonteleological thinking is without its dangers. It deprives man of his traditional props and thrusts

him out into a universe without a secure foundation of faith, into a life without assured meanings. But it also has its singular compensations in that it enables one to see the picture whole, free from the compulsion to love or hate.

III

The most amazing reaction to the scientific outlook is the recent charge that the destructive eruption of the irrational was caused, of all things, by the work of the scientists. Arthur Koestler, in *The Yogi and the Commissar,* declares: "To a certain extent the revolution in physics has certainly affected the artist, the revolution in psychology has influenced the political outlook, and similar cross-influences are easy to discover." And he correctly discerns the beginnings of a countermovement against materialism and rationalism, a vehement protest against scientific determinism. Koestler, at odds with the mechanical Marxists, indicates that the wholehearted acceptance of determinism involves a contradiction, for on the psychological plane man indubitably experiences an authentic sense of freedom. Scientific determinism, it seems, culminates in a cultural crisis, for it reduces man to the status of an automaton, the inevitable end product of the twin forces of heredity and environment.

In the conflict between science and literature, the very way in which the issue is phrased presupposes an ineradicable antagonism of attitudes and interests, an irrepressible war of values. Actually, if we stop to examine the situation without any prepossessions, we discover that psychologically there is no inherent incompatibility between belief in the scientific method and faithful devotion to the arts. Failure to recognize this simple fact works havoc with the literary conscience and drives many writers to adopt some spurious metaphysical or mystical system. What makes them wrestle with this seemingly intractable problem is the force of their need to achieve some kind of harmonious integration of their impulses, to get rid of anomalies and inconsistencies in their thinking. The conventional solution of the problem—namely, that science is the source of objective truth born of the discursive intellect whereas art is the product of the imagination and has aesthetic pleasure as its primary object—will no longer serve the purpose. For science, too, in its own way, provides nourishing food for the imagination.

In its highest reaches, the scientific mind is essentially creative, even though it is held in check by strict methods of verification. There are perplexities, enigmas, uncertainties, emotional crises, contingencies, for which science has no answer and which it is not at present equipped to resolve. The doubt remains: human loneliness and the piercing stab of pain, the burden of time, the last inglorious journey to the grave. Works of art can furnish their own emancipating vision into the heart of things, voice their own commentary on the unintelligible mystery. In the end, when the scientist-philosopher feels impelled to make some sense of all the data accumulated by physiology, symbolic logic, biology, anthropology, geology, chemistry, astrophysics, and bacteriology, he must fall back upon his imagination, he must depend on the poetic myth as the means of unifying his vision and affirming his faith, whatever it may be. But the point is that he unifies his vision and affirms his faith without running blindly counter to the massive, incontrovertible evidence that the best minds have patiently brought forth through the ages.

The ultimate lesson of science, like that of all true religion, is renunciation. In "A Free Man's Worship," Bertrand Russell declares that each of us must learn to accept the fate that stalks us with the inevitability of death. Between fact and fantasy, wish and fulfillment, ideal and reality, there is a gulf that cannot be bridged. But wisdom transcends despair by an act of creative imagination, by discovering beauty in the nonhuman world and in its own tragic predicament. This is a world we never made, but while we are living in it we can try to master it by penetrating as far as possible into the heart of the cosmic mystery. That is how the scientific mind confronts the common doom of mankind. Even though it is printed as prose, Bertrand Russell states his faith in a passage that is unmistakably poetry:

> Brief and powerless is Man's life;
> On him and all his race the slow
> Sure doom falls pitiless and dark.
> Blind to good and evil, reckless of destruction,
> Omnipotent matter rolls on its relentless way;
> For Man condemned today to lose his dearest,
> Tomorrow himself to pass through the gates of darkness,
> It remains yet to cherish, ere yet the blow falls,
> The lofty thoughts that ennoble his little day;
> Disdaining the coward terrors of the slave of Fate,
> To worship at the shrine that his own hands have built;
> Undismayed by the empire of chance, to preserve
> A mind free from the wanton tyranny that rules
> His outward life; proudly defiant of the irresistible forces
> That tolerate for a moment, his knowledge and his condemnation,
> To sustain alone, a weary but unyielding Atlas,
> The world that his own ideals have fashioned
> Despite the trampling march of unconscious power.

A Survey of Geophysics

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THE planet on which we live is one of the most interesting and beautiful subjects of research in applied physics. The science dealing with the physical properties of the earth as a whole and of its parts is called geophysics. In order to get an idea of its manifold aspects we may subdivide it from different viewpoints. With respect to the subject itself, there are three chief parts: the physics of the solid part; the physics of the liquid part, called oceanography; and the physics of the gaseous part, or physics of the atmosphere. This article will deal chiefly with the physics of the solid part, or geophysics, strictly speaking, which may be further subdivided into five branches.

1. Physics of the *solid earth as a whole*: This deals with the motions of the earth in space as a planet; with the form of the earth and its variations; with the elasticity and strength of the globe; with the density, pressure, temperature, viscosity, etc., in the interior of the earth; with terrestrial magnetism; and with the electrical phenomena in the earth.

2. Physics of the *outermost shell*: We can assume that the interior of the earth is built up in homogeneous concentric shells, but we find, by means of gravity measurements and by earthquake waves, that an outermost shell, the thickness of which we suppose to be about 100 km at the most, is distinctly inhomogeneous. Below the mountains the material is less dense than below the plains, and below the continents less dense than below the oceans. The distribution of water and land over the surface of the earth is not random, but the continents are like plates swimming on the viscous substratum. Their mutual positions determine the shape of the oceans, and their migrations are in close connection with the big features of tectonics. The volcanic actions, too, take place in the outermost shell, and most of the earthquake foci are there.

3. Physics of the earth's *crust*: The outermost layers of the earth, with a total thickness of 10–20 km, can be called the crust of the earth. It is that part of the solid earth which is accessible for direct observation, particularly because of erosion, by means of which new layers are continually cut off. Direct observation is also possible in tunnels, mines, and boreholes. The crust is the field of the geologist's research. Starting from the results of direct observation, he tries to determine the structure of the crust by extrapolation. In the crust there lie the mineral deposits, such as oil, coal, ores, etc., which are prospected and explored by man. The geophysical methods are of continually growing importance as a means for prospecting the structure of the crust and especially for prospecting the mineral deposits useful to man.

4. Physics of the *subsoil*: The civil engineer is interested in the upper part of the crust, which we call subsoil, and which we suppose to be about 100 m deep. When the engineer projects some large work, such as a great building, a dam, a bridge, a street, or a runway, he has to consider the physical properties of the subsoil, with regard to settlement under load, with regard to the hydrographic situation, etc.

5. Physics of the *soil*: The subsoil is covered with a weathered layer of varying thickness, which we call the soil. On the one hand, its physical properties are important with respect to its aptitude for supporting the life of plants and animals; on the other hand, these properties are in many respects considerably different from those of the subsoil; therefore, the physics of the soil must often be carefully considered when the subsoil has to be prospected.

Another kind of subdivision of geophysics can be made in view of the final aims of geophysical work. Man's aspiration for more and more knowledge is the cause of every kind of physical research of the globe without regard to the possibility of practical applications in the present or the future. The purpose of this kind of research, called general geophysics, is to acquire a still more extensive knowledge of the physics of the globe. Opposed to

the aims of general geophysics are the aims of applied geophysics, which deals with the application of geophysical methods to practical questions of geology, mining, and civil engineering. The methods are worked out and apparatus constructed to meet not only scientific but also economic requirements, and geophysical work is done not only by scientific institutions but also by commercial firms, the exclusive aim of which is to do geophysical prospecting for practical purposes.

Another current subdivision of geophysics is analogous to that of physics. Each branch of physics, from mechanics, acoustics, thermodynamics, optics, theory of electricity, and magnetism to atomic and nuclear physics, is important to geophysics. Such a subdivision is more than any other used in applied geophysics, the main branches of which are the gravitational, the magnetic, the seismic, the electric, the geothermal methods, and the methods of radioactivity. A special branch is well-testing by means of electricity, radioactivity, and other methods.

The Interior of the Earth

Is the interior of the earth in a solid, liquid, or gaseous state? is often asked of the geophysicist, and a conclusive answer cannot easily be given. The mechanical behavior of the earth may be compared with that of a piece of pitch which at room temperature seems to be a solid body. With a hammer we can strike off splinters. If we wait a day, we see that the same piece of pitch has flowed as a very viscous liquid. The behavior depends upon the duration of the action of the forces. As to the earth, the rate of its ellipticity shows that it behaves like a liquid drop under the influence of steady centrifugal forces. But, if the axis of rotation migrates, the earth behaves like a rigid body. The migrations of the axis can be perceived as oscillations of geographical latitude. From the period of these oscillations (some more than 400 days), called Chandler's period, we can calculate the modulus of rigidity of the earth as a whole, which is twice the modulus of steel at room temperature.

In reality, there are in the interior of the earth two chief parts with distinctly different mechanical properties. The inner part is the central core, with a radius of about 3,450 km; the outer part is the mantle. The boundary of the core was discovered by the observation of earthquake waves which, at this boundary, are reflected and refracted, so that the core produces a shadow zone at the surface of the earth. The transverse earthquake waves run through the mantle as through a solid body, but up to this day no transverse waves running through the core have been observed. We may therefore conclude that the core, or at least its outer part, is, with respect to elastic waves, in a liquid state. The most probable hypothesis about the composition of the core is that it consists chiefly of iron, like meteorites. With that hypothesis we can also quite easily interpret the average density of the earth, which is about twice the average density of the crust. It is true that the theory of the transition of the earth, at the beginning of its existence, from a sphere of uniform solar matter to a state with an iron core and an oxidic mantle of much smaller density, gives rise to some objection; therefore, the hypothesis has been taken into consideration that the boundary of the core is not of a material but only of a physical nature, and that the interior of the earth still consists of unseparated solar matter. But in this case the behavior of the earthquake waves near the core can scarcely be explained; hence, the discussion of these hypotheses is not yet closed. On the whole, our knowledge of the interior of the earth is very incomplete, as can be seen from the following example. The hydrostatic pressure increases with depth and exceeds three million atmospheres at the center of the earth. Such a pressure has never been attained in a laboratory; therefore no experiment has ever shown us the behavior of matter under such circumstances as exist in the interior of the earth. Moreover, we do not really know the temperature there, which chiefly depends upon the content of radioactive matter.

Gravity

To determine the figure of the earth was originally the chief problem of higher geodesy. By figure of the earth we do not mean the form of its real surface, which is too irregular and variable, but the form of the equipotential surface of the gravity field at sea level, called the geoid. A pure geometrical measurement of the figure of the earth is insufficient; the trigonometrical measurements of altitudes are not exact enough because the oceans are insurmountable obstacles for such measurements. The only way to determine the figure of the earth with sufficient accuracy is a geophysical one, namely, to measure the gravity over the continents and oceans. Based upon the results of those measurements, the determination of the geoid is possible. It is a famous solvable boundary-value problem of potential theory.

Before using the results of gravity measurements, they have to be reduced to sea level. For this purpose the influence of the masses above sea level on gravity has first of all to be calculated and sub-

tracted, or, in other words, those masses have to be taken away (topographical and Bouguer's correction). Next, the gravity stations have, by calculation, too, to be displaced vertically down to sea level (free air reduction). The differences between the reduced results obtained by means of that method and the normal gravity values are the "Bouguer's gravity anomalies." If the whole earth were built up of homogeneous shells, the Bouguer's gravity anomalies would be zero everywhere. In reality there are many regions with large Bouguer's anomalies; for example, under the mountain chains there are large negative anomalies.

An interpretation which is obvious from the viewpoint of mechanics was first offered in the last century by Pratt and Airy. They advanced the idea of isostasy, i.e., the conception of the swimming equilibrium of the superficial layers on a viscous substratum, a conception which has remained fundamental for an accurate appreciation of all mechanical phenomena relating to the uppermost shell of the earth. For example, to explain the negative Bouguer's anomalies below the mountains, we may imagine the outermost shell to be subdivided into vertical prisms, with bases of equal size standing side by side without any friction and reaching down to the same level, say, 100 km below sea level. It is clear that in the case of a swimming equilibrium all prisms have the same weight. Therefore, the prisms relating to mountains have a mass surplus above sea level which has to be compensated by a mass defect below sea level. This mass defect is the cause of the observed negative Bouguer's anomalies. There is a way of rigorously controlling the accuracy of the hypothesis of isostasy. Instead of taking away the masses above sea level, in order to reduce the measured gravity values to sea level, we may displace these masses vertically down to below sea level in such a manner as to fill out the mass defects we find there (isostatic correction). In the case of isostasy, the gravity anomalies of mountainous regions have to vanish; as a matter of fact, they do so nearly completely. For isostasy holds good not only in mountainous regions, but, with few exceptions, throughout the whole surface of the earth, and the isostatic correction of gravity enables the geodesists to determine the figure of the earth with much more accuracy than was possible by Bouguer's anomalies. This accuracy depends upon our knowledge of the exact distribution of the mass defects below sea level.

Hypotheses about this distribution were advanced by Pratt, Airy, and others. All these hypotheses are more or less schematically based upon the actual or an average shape of the surface of the earth. But, if we assume that the crust has a finite rigidity, it is possible that the maximum of mass defect does not lie exactly underneath the top of a mountain chain (which is probable in case of an asymmetric genesis of the mountain chain). In this case none of the above-mentioned hypotheses enables us to remove the gravity anomalies. Today a more detailed discussion of the equilibrium of the outermost shell or of a lack of equilibrium has to abandon the schematic hypothesis of isostasy and take into account the strength of the crust and all available knowledge of the mass distribution below sea level. During the past few years several remarkable attempts to determine the strength of the crust have been made. Such research is very difficult because it requires not a little experience in the mechanics of elastic and viscous bodies, and because the physical properties of the outermost shell are not well known.

As to the necessary knowledge of the mass distribution below sea level, the results of gravity measurements are an indispensable basis. What is the suitable form of the results of those measurements? Have we to start from isostatic or Bouguer's anomalies? Theoretically both are correct. Owing to the indubitable superiority of isostatic anomalies for geodetic purposes, many geophysicists considered these anomalies as superior for all other purposes, too. But there is no doubt that Bouguer's anomalies are more suitable for the determination of mass distribution, because, contrary to the isostatic anomalies, they do not presuppose the knowledge of the mass distribution below sea level and therefore they show in a more direct manner the required distribution. The real determination of mass distribution, based on Bouguer's anomalies, presents difficulties enough.

It must be pointed out that in principle it is impossible to determine univocally a mass distribution in the interior of a body by means of measurements of the gravity field on the surface of that body, for there is a well-known fact of potential theory that in a finite portion of space there always exists an infinite variety of different mass distribution generating the same gravity field on the surface of the portion of space. This principal fact holds good for magnetic fields, too, and we must never neglect it while evaluating gravity or magnetic measurements. To restrict the infinite variety of the mass distributions, additional assumptions on the required distribution are needed. These assumptions must be supplied by the geologist. The more the geophysicist knows about the structure of the outermost shell of the earth, and especially about the

geologic structure of the country where the gravity or magnetic values are to be evaluated, the more he is likely to find a mass distribution which is a good approximation of the real distribution.

A further means of evaluation is to combine different geophysical methods: for example, a combination of the results of gravity measurements with seismic facts to a determination of the boundary below the Swiss Alps between the upper crustal layer (called sialic layer, from Si + Al), the average density of which we assume to be 2.72, and the substratum (called sima, from Si + Mg), of density 3.2. The depth of the boundary in the northern neighborhood of the Alps is about 20 km below sea level, determined by seismic data, whereas below the Alps the boundary sinks to 37 km, thus showing clearly the effect of isostasy. In evaluating the boundary, Bouguer's anomalies were used.

Great progress has been made in the construction of gravity meters. During the past twenty years the sensibility of the instruments has been increased by a factor of 100. At the same time the portability and the speed of observation have been much augmented; therefore, the instruments are now suited not only for regional but also for local gravity work. To take full advantage of the possibilities of the instruments, the altitudes of the stations have to be known with an accuracy of 10 cm, and the topographic effects have to be corrected with all possible care. To do that, the densities of the rocks must be known as exactly as possible. The measurement of the densities of rock samples in the laboratory is not very satisfactory because the effective density of the rocks also depends upon the pores and cracks and upon their filling. Recently the densities of rocks *in situ* have been calculated by means of the gravity values themselves in the following manner: Imagine a mountain, standing in a plain, with the gravity measured on a line of stations beginning in the plain, going over the mountain, and ending in the plain at the other side of the mountain. The subsoil is assumed to be homogeneous. The gravity values of the stations situated on the mountain should be reduced to the level of the plain. To do the reduction, a rock density of the mountain has to be assumed. By some trials we find such a value of rock density that the reduced gravity values are the same at all stations of the line. That rock density is the required one. The method needs further development and investigation in order to determine whether it may also be applied in really mountainous regions.

Terrestrial Magnetism

The compass needle as a simple and secure means of orientation has been known for many centuries. From a physical viewpoint the compass is a simple instrument measuring an element of the geomagnetic field. It was only a century ago that a mathematical analysis of the field was first given by C. F. Gauss. This great mathematician carried out the analysis in a really brilliant manner, and up to this day it has remained the only proceeding adequate to the problem; namely, he invented the potential theory, which in the course of the past hundred years has found so many applications in different problems of mathematical physics. A chief result of the analysis is the fact that the sources of the geomagnetic field, except perhaps a small residue, lie in the interior of the earth and not in the space outside the earth. A second result concerns the form of the field. Its main part is the field of a dipole, with an axis coinciding approximately with the earth's rotational axis. On this field a much smaller part is superimposed, causing the smooth variations of continental extent.

With our physical knowledge of today, we see principally two different possibilities to explain the physical nature of the sources of the geomagnetic field. The first possibility is to assume a permanent magnetization of the material within the earth. Such a permanent magnetization exists in the crustal layers because they contain ferromagnetic minerals like magnetite. The second possibility is to assume the existence of a system of permanent electric currents within the earth. Today, this second possibility is considered more probable, because a permanent magnetization in the required amount can hardly be imagined. If the temperature rises above the Curie point (about 600° Cels), the minerals lose their ferromagnetic properties.

But we are still far from having found a satisfactory explanation of the origin of the geomagnetic field by the aid of electric currents. Much more knowledge about the properties of material under the influence of very high pressures and high temperatures is needed. It is to be expected that the final solution of that problem will also clear up the origin of another geomagnetic phenomenon, namely, the secular variations. For example, the direction of the compass needle varies in the course of the centuries, slowly but steadily, by several degrees, without following an obvious law. We understand much better the origin of the other variations with time, which are transitory or periodic, with known periods. These variations are closely connected, partly with the motions of the ionosphere, partly with the solar activity, especially with the occurrence of sunspots. The greatest disturbances of the geomagnetic field, magnetic storms, are caused by charged particles

emitted by eruptions on the sun and, caught and guided by the geomagnetic field, precipitated into the earth's atmosphere, causing here the polar light phenomena and the disturbances of the ionosphere. These disturbances are, with regard to radio waves, of practical interest, and the steady observation of solar activity allows the prediction of the interruption of radio connections caused by disturbances of the ionosphere.

The variations with time of the geomagnetic field must be taken into account when magnetic measurements for the purposes of geology or mining are to be made. Another difficulty of such measurements is the shape of the magnetic anomalies, often extremely complicated. In the case of magnetized material at the surface of the earth, the anomalies depend very strongly on the topographical features, and, moreover, they vary within such a wide range that the drawing of a map with curves of equal values of the anomaly is quite impossible. In such a case the difficulty has been successfully removed by drawing the curves for values of a geometric instead of an arithmetic progression. If only a smoothed map is required, the measurement by airplane with the so-called airborne magnetometer is a beautiful and very expedient method. Compared with other methods of applied geophysics, the magnetic methods are easy to handle and not expensive. But it is often difficult to evaluate the results. Progress can be expected from further research upon the relations between the magnetic properties of rocks and their characteristic petrographic properties. Finally, we have to point out the necessity of knowing the places of strong magnetic anomalies in a country where traverse lines by aid of a compass have to be drawn. The strong anomalies must be carefully avoided.

Earthquakes and Applied Seismology

Whenever an earthquake occurs, it is a very impressive natural phenomenon. On the one hand, it stimulates man's imagination and his impulse to scientific research; on the other hand, it forces him to look for means of protection against earthquake damage. To discuss earthquakes and applied seismology, let us subdivide the whole.

Earthquake waves: Each earthquake originates from a place, called the focus, lying in the interior of the earth in a depth between 0 and 700 km. Compared with the size of the whole earth, we can consider the focus as a point. The cause of an earthquake is a rupture of the material at the focus, with the result that from the focus elastic waves radiate in all directions, like sound waves caused by an explosion. The elastic earthquake waves run through the interior of the earth and emerge at the surface, where they are recorded by seismographs. An earthquake record shows an extremely complicated mixture of waves of different amplitudes and wavelengths, and, when we look at such a record for the first time, we are tempted to consider a great part of these complications as an image of intricate motions at the focus during the rupture. But this conception is incorrect, which is shown by the following fact. Often the records of two earthquakes registered by the same station, originating at the same focus, are nearly identical. From this fact we conclude that the motion of the material during the rupture at the focus is a relatively simple shock during only a very short time (some seconds, or even less), and, thus, that the complications and the length of the record (amounting to some hours) are due, so to speak, to the "fate" of the waves on their way between the focus and the earthquake stations.

By reading such records, much important information about the structure of the earth's interior has been obtained. All other geophysical methods are far from getting information of the same accuracy. Nevertheless, earthquake records have remained cryptograms of which we are able to decipher only a small part. Our own and future generations will have a difficult but interesting task in trying to solve the problem of reading earthquake records completely. Today, the chief data that we get from the records are the moments of arrival of some wave groups. Compiling the records from all stations of the world and comparing those moments, we get the travel-time functions from which the exact position of the focus, the exact focal time, the depth of discontinuities, etc., can be calculated. In this way, the earth's central core and the depth of the boundary between the sialic layers and the simatic substratum have been determined.

The depth of this boundary is not the same over the whole earth. Further investigations by means of near earthquake records are needed in order to draw a more complete picture of the outermost shell throughout the world. Especially are the regions about the Tertiary mountains and the margins of the oceans of interest, on account of the relations to the gravity anomalies, the volcanic activity, and other geophysical problems of first order. The compiling of all available travel-time data is much needed, for these data are fundamental for most further seismological research.

A very large field of present and future research is the physics of earthquake waves. Acoustics deals with longitudinal waves, optics with transverse waves, seismology with both kinds, propagated in the same material with different velocities. At each

boundary, an incident wave of one kind is split off into different waves, or, in other words, at each boundary different waves are coupled. Moreover, surface waves much more complicated than the longitudinal and transverse bodily waves run along the boundaries. It is easy to imagine that the mathematical treatment of these waves is an interesting but difficult branch of mathematical physics. For example, Laplace's transformation has been successfully applied to some problems of wave propagation in a homogeneous solid half space. Theoretical and experimental research of damped waves, which presents additional difficulties, has also been done, and we may consider that research a promising beginning of further work in this field.

About focal motion: Although travel-time methods are used more than any other in seismology, there are a number of other methods leading to further results concerning earthquakes. With regard to the cause we have to distinguish between different kinds of earthquakes. By the activity of circulating water, cavities can be formed in certain layers of the subsoil. The falling-in of such a cavity can be the cause of an earthquake. Earthquakes are connected with volcanic activity, too. But all these earthquakes have only small energies, and they are at the most felt near the focus. Only the third kind of earthquakes—the tectonic earthquakes—can release great energy. Owing to the mobility of the earth's outer shells, there are definite regions where strain energy can be accumulated gradually. If a certain maximum energy, depending upon the local circumstances, is reached, the energy is suddenly released by rupture, causing a tectonic earthquake. The energies of the greatest of the known earthquakes have been estimated to be more than 10^{26} ergs.

The depth of the focus varies from 0 km to more than 700 km. The deep-focus earthquakes deserve the special interest of the geophysicist. Their existence is another proof of the fact that the material in the interior of the earth, down to 700-km depth, at the same time behaves like a liquid (as substratum of the swimming continents) and like a solid body (allowing ruptures). The fact that the maximum energy observed for the strongest deep-focus earthquakes is much less than the maximum energy of the normal earthquakes is comprehensible. The foci of the deep-focus earthquakes are situated exclusively at the margin of the Pacific Ocean, showing that this margin is a structural line of first order in the mantle of the earth, which is also confirmed by other facts of geophysics. The Pacific is the unique region where a sialic layer is missing, or at least very thin.

As to details of motion at the focus, our knowledge is still limited. In special cases the position of a fault-plain of the focus could be found, but to increase our knowledge of focal motion, more earthquake stations in regions of intensive earthquake activity are necessary; moreover, a special adaptation of the seismographs to such research will be useful. Many efforts in this direction have been made, but much more remains to be done, and may be done by using electronics in a more effective manner.

True ground motion: In order to determine the true ground motion by seismograph records, the seismograph has to respond to ground motion with much greater accuracy than when only travel times are to be determined; furthermore, the hypothesis customarily implied that the ground motion is practically of translatory character is probably not sufficiently exact, if true ground motion in the neighborhood of the focus is dealt with. The response of the seismographs to rotational motions must be carefully taken into consideration, too. The knowledge of true ground motion near the focus is indispensable for the civil engineer when he deals with the problems of earthquake-proof construction. Research on the effects of earthquake motion upon buildings presents difficult apparative as well as mathematical problems, and we are still far from definite solutions.

Macroseismic research: This is a field of earthquake research which seems to be of decreasing interest; nevertheless, it is worth mentioning. Macroseismic research means research without the aid of instruments. There are always problems that need direct human observation, such as research on earthquake damage. Moreover, the number of earthquake stations is nowhere sufficient to give us a complete picture of earthquake motions in all those regions where earthquakes are felt. If a part of the subsoil consists of sedimentary layers, the intensity of earthquake motion, and therefore the damage, can vary considerably in one and the same country; and if new buildings are planned, the quality of the subsoil must be considered.

A basis for macroseismic research is the sensitivity of the human body to ground motions. Knowledge of that sensitivity is got by comparing the effects of earthquake motion upon the human body with seismic records, and by the aid of shaking-table experiments with people. The results of such research are interesting not only with regard to earthquakes: people are also subject to motion as passengers in vehicles of all kinds, such as ships, railway trains, and airplanes, and the effects of the motion, such as nausea, should not be ignored.

A popular topic of discussion is earthquake predicting, but in spite of much research and many trials, no serious basis for the prediction of earthquakes has so far been found.

Seismic prospecting: As in earthquake research, in seismic prospecting, too, travel-time methods are of utmost importance. The geometry of seismic waves, especially their refraction and reflection at discontinuities in the crust, and the corresponding technique of measurement have been developed to an admirable extent, above all in the United States in connection with prospecting for oil. With a method much different from the usual one the plains of northwestern Germany have been prospected. Over the whole region, the travel times of refracted seismic waves for a standard distance of 4 km have been measured and mapped. The result is a very instructive picture of the subsoil structure, especially if it is combined with the gravimetric and the magnetic maps that have been made out for the same region.

In mountainous regions, such as Switzerland, the seismic methods are developed and used in a manner adapted to the special circumstances and problems of the country. Apparatus is easily transported without motor trucks, and often seismographs with more than one component are used. On the one hand, the geometry of seismic waves is more complicated than in the plains. Therefore, the methods of interpretation have often no standard form, but each problem has to be solved in its own way. On the other hand, solution of a problem is often facilitated by the fact that the rocks of the different layers admit of direct approach, and thus direct measurement of the velocities of the elastic waves is possible. A frequent problem is the seismic determination of rocky profiles covered with alluvial materials, in narrow valleys, in connection with the planning of power stations. Another group of problems frequently dealt with in Switzerland is the seismic prospecting of glaciers in the Alps. The knowledge of the form of the rocky surface below the glacier is very important in glaciology, and unexpected results have been found—e.g., the rocky surface has somewhere a deep basin filled with "dead ice" and covered with moraine, on which the glacier is lying.

Further Topics

With regard to the variety of methods and results, the electrical phenomena form a large branch of geophysics. In the interior of the earth electrical currents are induced by the temporal variations of the geomagnetic field. Another worldwide electrical phenomenon is the direct current flowing vertically through the surface of the earth (the negative loads going downward) and causing a potential gradient of about 100 $\frac{\text{Volt}}{\text{m}}$ in the air near the surface. Today it can be taken for granted that this current is maintained by the transport of electrical loads supported by the lightning during thunderstorms. Local steady potentials are due to ore bodies the upper parts of which lie in the weathering zone near the surface and therefore act as a galvanic element. A great number of different methods are used in electrical prospecting. The most frequently used method is the measurement of the earth's resistivity by direct current. But exact values of the thickness of layers in the subsoil cannot always be received, because the mathematical treatment of the electrical field in stratified media is complicated and so far no practical solution has been found. The theoretical difficulties increase if alternating current of low frequency or even of high frequency is used. Therefore the interpretation of geoelectric measurements requires experience. A special electrical method, used throughout the world, is the electrical survey of drill holes, today supplemented by similar methods using radioactivity generated by neutrons.

An interesting application of electrokinetic effects is the acceleration of water flow in the subsoil, by an electrical field, in order to dry up a dam or to increase the yield of a water well.

There are many problems in which different branches of natural science are connected with geophysics—these we shall only briefly mention. The earth's thermal state and history are on the one hand connected with the problems of cosmogony, and on the other hand with the problems of the concentration of radioactive matter in the earth's interior, a problem common to geophysics and geochemistry. A similar case is the determination of the age of the earth by the aid of the natural radioactivity of matter.

Our survey will be closed with the remark that geophysics offers a beautiful opportunity for close cooperation of the different branches of natural science, above all of mathematics, physics, and geology.

Research Studies in Mineral Engineering Using Radionuclides

A. M. GAUDIN and P. L. DE BRUYN

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ONE of the greatest scientific advances of the past half century is without doubt the discovery of radioactivity and, more specifically, artificial radioactivity. Rapid improvement in technical methods for the production of artificial radioactive elements has made available a large number of radionuclides. Scientists have been employing this new tool to an ever-widening degree to elucidate chemical, biochemical, physiological, and pathological problems. But as yet engineers do not seem to have grasped sufficiently the extent to which tracers can help them understand the processes with which they deal and thus become of actual practical use to them.

Mineral engineering is a broad field of technology that deals with an immense variety of substances, mostly inorganic, most frequently but not exclusively solid, and often in a state of fine subdivision. The problems in this branch of engineering are of such a nature that radionuclides can be used effectively.

This article is concerned with the application of radionuclides to mineral dressing, and particularly to the detailed mechanism of the flotation process of mineral separation. A brief discussion of some typical problems to which this tool is applied is included. These examples are taken from current research programs in progress at the Massachusetts Institute of Technology under the sponsorship of Armour and Company and the Division of Research, United States Atomic Energy Commission. No attempt will be made to discuss in detail the nuclear physical and radiochemical principles which define and govern the phenomenon of radioactivity. Excellent discussions of these topics have been offered in the current scientific literature.[6, 12, 27] Methods of measurement and instrumentation are presented only very sketchily, since this topic is also well covered in current publications.[9, 26, 29, 41]

As is well known, the tracer method has two remarkable properties. One is the extreme sensitivity of the method; the second is the ability to trace individual atoms among their own kind. Both properties have been used in mineral dressing studies, particularly in flotation. At present apparently the analytical qualifications of radionuclides are in greater demand than their tracer qualifications.

The Flotation Operation and its Characteristics

The object of the flotation procedure is to separate mineral fragments from one another. This separation is accomplished in the following manner. The mineral mixture is made into a pulp with water to which various agents are added. Most of these agents are in solution. Molecules, or ions, of the hydrocarbon-containing "collecting" agents attach themselves to a mineral, decreasing the surface free energy of the solid and rendering its surface less water-avid. As they come in contact with coursing air bubbles in the liquid suspension and adhere to them, the hydrocarbon-coated particles are carried to the air-liquid surface, where they are collected in the froth of air bubbles. This froth is stabilized not only by its solid load, but also by the presence of a nonionizable, soluble surface-active "frothing" agent already added to the pulp. The mineral-laden froth is then mechanically removed

from the system. The collecting agent is always a heteropolar organic compound that has to be soluble in water to some extent.

Only since about 1920 has mineral separation by flotation become widespread. The art has been developing rapidly, meeting the need for cheap methods of concentrating low-grade ores. The theory of flotation, however, has lagged far behind the practical art. A flotation system is complex, owing to the large number of phases present. The main interest lies in the compositional variations at the phase boundaries, the equilibrium relations with the bulk liquid phase, and the effect of compositional changes at such interfaces on the relative floatability of the solids.

Any flotation system has a minimum of three phases, the solid, liquid, and gaseous. The solid phase has a crystalline structure, which may be complicated, depending on the chemical composition of the mineral. The number of solid phases may be large and is fixed by the mineral associations in the ore to be treated. The surface of the mineral is of greatest importance, and the thermodynamic properties of this surface which are of major interest are its total surface and surface free energy. Change in the surface free energy can be considered to be the driving power in the flotation operation, but a direct measurement of this change is very difficult, if not impossible. Such surfaces are characterized by varying degrees of specific valency unsaturation, depending on the chemical composition of the mineral. The polarity of a surface can be interpreted in terms of the rupturing of interatomic bonds during its creation.

In accord with the Langmuir concept of chemosorption, a surface can be regarded as consisting of a variety of two-dimensional sites whose parameters are determined by the crystal structure of the mineral. In the vicinity of the surface, an electric field may be thought to exist due to the unsaturated forces of the surface atoms; this field "explains" the attraction of foreign atoms and molecules to the surface. Upon immersion in water, the surface of most minerals is not expected to remain unaltered, since there is an attraction of the reactive sites for the polar water molecules and other ionic species in the medium. A "clean" surface is difficult to realize, to visualize, and to define.

The liquid phase of the flotation system is characterized by its multicomponent composition as determined by dissolution products produced at mineral surfaces, the presence of soluble salts in the mineral mixture, and the sundry reagents added in order to effect and control the mineral separations. The physical-chemical relations and equilibria among these components in the solution have a direct effect on mineral surface-aqueous phase interactions. The gaseous phase can vary considerably in composition without affecting the operation, provided the gases are not too soluble in the aqueous solution. Air is the most commonly used medium; it may complicate flotation because of its oxidizing potentialities.

The usual analytical tools, even spectrographic analysis, are often inadequate to determine the distribution of flotation agents between the mineral surface and the solution phase, and the air-water surface and solution phase. They also yield no information on the mechanism of self-diffusion of the agents in the system. It must therefore be recognized that, although ordinary chemical analysis and spectrographic analysis have their place in mineral engineering researches, there are many problems where they cannot begin to supply answers obtainable through the use of radionuclides.

Limitations of Radionuclides in Mineral Engineering Research

An atom is radioactive if at some future time its nucleus will disintegrate spontaneously. The specific time at which this decay is to occur is not known, but the statistical rate at which nuclei of some particular kind decay is generally known. The decay involves great, sudden, and localized energy changes. Measurement of radioactivity can be made either by counting the events or by determining the sum of the electrical effects of all the events within a given period of time. Both methods of measurement have been used, the first more widely on account of its greater sensitivity. Thus assay for carbon 14, the very convenient radionuclide of carbon, can be made by counters or with ionization chambers.

The over-all efficiency of a counter depends on the geometry of the setup; it depends also on the absorption of the radiation from the point of emission to the measuring apparatus. In practical mineral engineering problems this efficiency may become high enough to be near 100 per cent or low enough to equal not more than a fraction of 1 per cent.

The synthesis of compounds containing radionuclides may involve complicated chemical procedures. One should therefore expect a lower percentage of the labeled atom in the synthesized product than in the material from which the final compound was processed. The original compound which contains the desired tracer atom has a maximum activity per unit weight, measured in terms of millicuries per gram, which is always less than

the activity that would prevail if all the atoms of the desired element were labeled.

Radioactive carbon is supplied by the United States Atomic Energy Commission's laboratories at Oak Ridge in the form of barium carbonate, which usually contains 1–3 labeled carbons per hundred carbon atoms. From this source Harwood and Ralston,[22] of Armour and Company, synthesized for use at the MIT mineral engineering laboratories various amine salts. The most concentrated of these preparations has a labeled carbon content of only 0.00067 per cent. This represents a dilution of activity of some five thousandfold from the radiocarbon source supplied by Oak Ridge. Even though a relatively small number of the amine radicals contain labeled atoms, and even though radiocarbon is long-lived (half-life of 5,700 years), so that few of the active atoms decay during an experiment, it is nevertheless possible to detect with ease the presence of a few micrograms of the amine salt. With the available percentage of radiocarbon in the barium carbonate, it should, moreover, be possible to synthesize substantially more active amines than those used so far.

For the quantitative analysis of a chemical compound in very low concentrations an important pre-radioanalysis step may be required. Quite often the accuracy and reliability of the final result depend on this step. For example, in order to determine the amount of radioactive dodecyl ammonium acetate in a dilute water solution, it was found necessary to concentrate the solution by selective distillation of the water while keeping the amine in as insoluble a form as possible, before the radioanalysis was attempted. This step is required because the activity of the solution cannot be determined directly on account of the low decay energy of C^{14} and the large absorption of the decay radiation by the aqueous solution and the wall of the counter. Although the detection method employs internal gas counting, which has the highest sensitivity, the accuracy of the result should be gauged by the success obtained in retaining all the amine in the distillation step. In this particular illustrative example, two steps are involved before the final radioanalysis can be made, since after the amine has been concentrated a combustion procedure is also required to convert the carbon in the amine to carbon dioxide. Experimentation has shown that the combustion step yields a recovery of over 99 per cent of the oxidized carbon, provided suitable care is taken.[14]

Determination of the adsorbed amount of a specific substance on a mineral sample would be relatively easy and foolproof were it always possible to place the mineral with its adsorbed film under a window-type Geiger-Muller counter or in a gas flow counter. Unfortunately, in many cases the radiodetermination is not so simple. If the total measurable activity on the surface is extremely low —that is, if it is comparable to background radiation or if the radioelement has such a low decay energy that absorption of the radiation is large— then it is advisable to remove the adsorbed film from the surface and introduce it in some form or other into a more sensitive detection instrument. Here, again, the accuracy of the analysis will be controlled by the success with which the film can be removed from the surface, as well as by the nature of the chemical form of the tracer atom after removal.

A problem which may be of importance is the likelihood that a tracer atom in a given compound could exchange with a nonradioactive isotope in a substance with which it is physically associated. It has been shown that in a radioactive alcohol-water solution where the tracer atom is tritium (H^3), exchange does take place between the tritium of the alcohol and the hydrogen atom (H^1) in the water molecule (27, p. 414). The presence of radioactive carbon dioxide in the atmosphere immediately above solid radioactive barium carbonate has also been reported as evidence that such exchange does occur.[1, 46] In the study of the adsorption of dodecyl amine by quartz, it has been established experimentally that an adsorbed layer of radioactive amine in equilibrium with a given concentration of this radioactive salt solution in a column of quartz can be completely replaced by inactive amine, by passing a solution of similar strength but containing the nonactive salt through the column.[13] The reverse of this process has also been found to be true. This simple but nevertheless important experiment removed any doubt as to the similarity in the adsorption behavior of radioactive and nonactive amines and in addition showed that amine adsorption is a reversible process. These examples illustrate three different kinds of exchange, of which the first two are undesired phenomena capable of vitiating otherwise well-planned experimental campaigns, but the third represents establishment of an experimental fact important to mineral dressing.

Desirable properties of radioelements for use in mineral engineering are a relatively long half-life (measurable in terms of at least days, and preferably weeks, months, or years) and a high kinetic energy of the radiated particle or wave. Generally speaking, detection of betas is preferred if possible. Another requirement which in general is not appreciated is that simple or single-step decay is de-

sirable, since this introduces no complexity due to the activity of the daughter radioactive product(s).

Often a specific tracer element does not have a favorable balance of these properties. In the case of beta-emitting nuclei of the natural radioactive series, Sargent has found that there is a constant decrease in maximum energy of the beta ray with increasing half-life or decreasing decay constant (27, p. 174). Carbon 14, which is a beta emitter, has a half-life of approximately 5.7×10^3 years, with a maximum energy of only 0.154 Mev. For such a radioelement, the more sensitive detection instruments have to be used. The use of radiation having a long range of absorption requires care in handling, and health protection for the personnel, at least when substantial activities are used. Gamma rays and neutrons are examples. They can be detected only by virtue of secondary effects produced by interaction with matter, and in this case the choice of detection instrument will be more limited than in the case of beta particles. The efficiency of gamma counting is usually low, except when scintillation counters are employed.

The advantages and disadvantages of the tracer method for a specific research problem have to be weighed before using it. So many complications and technical difficulties may arise that the method may not be wise at all. Furthermore, in order to be effective the experimenter should have a certain basic knowledge of the principles of nuclear chemistry and of operational characteristics of radiodetection instruments. Such a training is not to be neglected, for the same reason that it would be agreed that any technical chemical analyst should be familiar with the principles of analytical chemistry.

Table 1 presents a list of the radioelements that are best suited for tracer studies in mineral engineering. The nuclides are arranged in order of increasing atomic number. All the physical data incorporated in this table have been taken from the Trilinear Chart of Nuclear Species as compiled by William H. Sullivan and published by John Wiley and Sons, Inc., New York (1949).

Twenty-nine elements and 34 radionuclides are included in this table. Three of the 34 radionuclides occur in nature—Pb^{212} and Bi^{210} belonging to the Th^{232} and U^{238} decay series, respectively, and K^{40}. The natural radioactive isotope of potassium has no tracer qualifications, due to its extremely long half-life, but has been included since it has been shown that during the concentration of potassium-bearing minerals, especially sylvite, assays of the potassium content of the concentrate and tailings can be made radiometrically.[15]

There are two major reasons for including in certain instances more than one radioisotope of the same element:

1. Many elements have more than one stable isotope, and, even though just one isotope is required to produce the desired radionuclide by bombardment with neutrons, deuterons, etc., other nuclides will be produced in the process too. Fe^{59} is produced in the pile at Oak Ridge by bombarding an iron target with neutrons according to the following reaction:

$$Fe^{58} + n \longrightarrow Fe^{59} + \gamma + \text{heat}.$$

Since iron has four stable isotopes ($Fe^{54, 56, 57, 58}$), Fe^{55} is also produced by this (n, γ) reaction. Likewise, Mn^{54} is usually produced in the cyclotron by bombarding an iron target with deuterons ($_1H^2$), but Mn^{52} appears as a product at the same time. Sn^{121} and Sn^{123} will also be produced together by the (n, γ) reaction if stable tin is target material.

2. An experiment may last for several days, and it would be convenient to use the longer-lived of the suitable radioisotopes in order to avoid too many corrections for decay during the experiment. This is the reason why both Na^{22} and Na^{24} are included in the table. The reason why very short-lived radionuclides have not been included in this list is that in order to use such nuclides the place of research has to be located at the pile or cyclotron. Some elements, such as gold and cobalt, for instance, have other nuclides in addition to those listed which are suitable for research, but these have no added advantage as far as research in mineral engineering is concerned.

Most of the nuclides listed in the table are available from the Oak Ridge Laboratories. The few exceptions are those which are not in demand for research purposes. An attempt has been made to indicate the best methods for detecting the decay of the nuclides, and references are given wherever possible.

Use of Labeled Atoms in Some Flotation Problems

The problems which are to be studied with the assistance of tracers in connection with the attachment of collectors (e.g., alkyldithiocarbonates, alkyldithiophosphates, mercaptans, fatty acids, amines, etc.) to mineral surfaces can be summarized briefly. If attachment of the collector to the mineral is accomplished by a process of adsorption of the collector in an oriented position onto preferred sites, it is of theoretical interest to determine the type of adsorption reaction that is involved in any special system. Is this adsorption due to an exchange between the collector molecule and the lattice atoms or an exchange between a collector ion

TABLE 1
LIST OF RADIONUCLIDES FOR USE IN MINERAL ENGINEERING

Legend:

s = second
m = minute
h = hour
d = day
y = year

β^- = beta ray
β^+ = positron
γ = gamma ray
α = alpha ray

e^- = internal conversion electron
K = K capture
* indicates that particular nuclide exists as two or more isomeric states; the table includes only those state(s) having a suitable half-life.
† indicates that nuclide is naturally radioactive.

Nuclide	Decay Scheme	Half-life	Energy of Radiation Mev.		Detection Method
			Particles	γ-rays	
C^{14}	$_6C^{14} \xrightarrow{\beta^-} {}_7N^{14}$	5.7×10^3y	0.155	No	Gaseous and solid beta counting;[6] internal gas counting; proportional counter; end-window G.M. counter; ionization chamber.
Na^{22}	$_{11}Na^{22} \xrightarrow{\beta^+\gamma} {}_{10}Ne^{22}$	2.6y	0.58	1.30	Solution and solid beta counting;[25,35] window-type G.M. counter; solution G.M. counter.
Na^{24}	$_{11}Na^{24} \xrightarrow{\beta^-\gamma\gamma} {}_{12}Mg^{24}$	14.8h	1.390	1.38_0 2.75_8	
P^{32}	$_{15}P^{32} \xrightarrow{\beta^-} {}_{16}S^{32}$	14.3d	1.71_2	No	Solution and solid beta counting;[7,25,42] detection very simple due to hard beta. Thin-window G.M. tube; solution G.M. counter
S^{35}	$_{16}S^{35} \xrightarrow{\beta^-} {}_{17}Cl^{35}$	$87._1$d	0.169		Solid beta counting; detection methods same as for C^{14} with exception that no internal gas counting is done.[11,24,25,47]
Cl^{36}	$_{17}Cl^{36} \xrightarrow{\beta^-} {}_{18}A^{36}$	$\sim 2 \times 10^6$y	0.6_6		Solid counting with thin-window G.M. tube; possibly also counting in the gaseous state, using a high sensitivity electrometer ionization chamber or modified Lauritzen electroscope.[24,25]
† K^{40}	$_{19}K^{40} \xrightarrow{\beta^-}$ (90%) $_{20}Ca^{40}$; $_{19}K^{40} \xrightarrow{K,K\gamma}$ (10%) $_{18}A^{40}$	$\sim 1.4 \times 10^9$y	1.5	1.5_4	Solid and solution beta counting;[15,24,25] K^{40} detection is done with a thin-walled G.M. tube. This method is used in assaying for K in halite-sylvite ores. K^{42} detection is similar to Na^{24}.
K^{42}	$_{19}K^{42} \xrightarrow{\beta^-\gamma;\ \beta^-} {}_{20}Ca^{42}$ (1) (25%) (2) (75%)	12.4_4h	(1) 2.0_7 (2) 3.5_8	1.5_1	
Ca^{45}	$_{20}Ca^{45} \xrightarrow{\beta^-} {}_{21}Sc^{45}$	15_2d	0.26	No	Beta counting in solid form;[2,20,24,25] proportional counter; end-window G.M. counter.
Cr^{51}	$_{24}Cr^{51} \xrightarrow{K,\gamma,e^-} {}_{23}V^{51}$	26.5d		0.32_0 (~3%)	Thin-walled gamma counter.[4,5,24,31]

*Mn^{52}	$_{25}Mn^{52}\ \beta^+\gamma\gamma\gamma$; K $\rightarrow\ _{24}Cr^{52}$ (1) (35%) (2) (65%)	$6._5$d	0.58	0.73 0.94 1.46	Gamma counting. [24,25,39]	
Mn^{54}	$_{25}Mn^{54}$ Kγ $\rightarrow\ _{24}Cr^{54}$	31_0d		0.83_5		
Fe^{55}	$_{26}Fe^{55}$ K $\rightarrow\ _{25}Mn^{55}$	~4y			Beta counting of Fe^{59}; thin-mica window G.M. counter. Counting of X-rays emitted by Fe^{55} and Fe^{59}; Beryllium-window G.M. counter. [21,24,25,34]	
Fe^{59}	$_{26}Fe^{59}\ \beta^-\gamma$; $\beta^-\gamma\ \rightarrow\ _{27}Co^{59}$ (1) (50%) (2) (50%)	46d	(1) 0.26 (2) 0.46	1.30 1.10		
*Co^{60}	$_{27}Co^{60}\ \beta^-\gamma\gamma\ \rightarrow\ _{28}Ni^{60}$	5.3y	0.31	1.16 1.31_7	Beta counting; [8,24,25,39]	
Cu^{64}	$_{29}Cu^{64}$ β^- (39%) $\rightarrow\ _{30}Zn^{64}$; K(42%); K$\gamma$(0.5%); β^+(18.5%) $\rightarrow\ _{28}Ni^{64}$	12.8_8h	0.574(β^-) 0.658(β^+)	No No 1.35	Beta counting; thin-window G.M. tube. [8,18,24,25]	
Zn^{65}	$_{30}Zn^{65}$ K; Kγ; $\rightarrow\ _{29}Cu^{65}$ (50.5%) (48.5%)	25_0d		1.12	Gamma counting. [19,24,25,28]	
As^{74}	$_{33}As^{74}$ β^-(15%); β^-(15%) $\rightarrow\ _{32}Ge^{74}$; K$\gamma$(35%); β^+(35%) $\rightarrow\ _{34}Se^{74}$	18d	0.7_2(β^-) 1.40(β^-) 0.95(β^+)	0.58_8	Beta counting. [24,25]	
As^{76}	$_{33}As^{76}\ \beta^-\gamma$;$\beta^-\gamma$;$\beta^-\ \rightarrow\ _{34}Se^{76}$ (1) 15% (2) 25% (3) 60%	26.8h	(1) 1.3 (2) 2.5 (3) 3.05	1.75(2%) 1.2(13%) 0.55(38%)		
Br^{82}	$_{35}Br^{82}\ \beta^-\gamma\gamma\gamma\ \rightarrow\ _{36}Kr^{82}$	35h	0.465	0.54_7 0.78_7 1.35	Beta counting. [24,25,32,36]	
Sr^{90}	$_{38}Sr^{90}\ \beta^-\ \rightarrow\ _{39}Y^{90}\ \beta^-\ \rightarrow\ _{40}Zr^{90}$ (A) (B)	(A) 25y (B) 62h	0.61 2.3_5	No No	Beta counting; thin window G.M. tube. [24,25,40]	
Mo^{99}	$_{42}Mo^{99}\ \beta^-\gamma\ \rightarrow\ ^*_{43}Tc^{99}\ \beta^-\ \rightarrow\ _{44}Ru^{99}$ (A) (B)	(A) 67h (B) 10^6y	0.24 1.3 0.3_2	0.24,0.77 0.81_5,0.84 No	Beta counting of hard beta rays of Mo^{99}. [7,43]	
*Ag^{110}	$_{47}Ag^{110}\ \beta^-\gamma\ \rightarrow\ _{48}Cd^{110}$	27_0d	0.09 0.59	0.116,0.65_6 0.88_5,0.93_5 1.52	Beta counting (solid and solution); [25] thin-window G.M. tube	
*Cd^{115}	$_{48}Cd^{115}\ \beta^-\gamma\ \rightarrow\ _{49}In^{115}$	43d	1.67	~0.5	Beta counting; should be no difficulty counting hard betas	
*Sn^{121}	$_{50}Sn^{121}\ \beta^-\ \rightarrow\ _{51}Sb^{121}$	28h	0.4	No	Double isomerism for Sn^{121}; triple isomerism of Sn^{123}. Beta counting; counting of Sn^{123} should be easier.	
*Sn^{123}	$_{50}Sn^{123}\ \beta^-\ \rightarrow\ _{51}Sb^{123}$	10d 130d	2.48 1.5	 No		

$^{m}Sb^{124}$	$_{51}Sb^{124} \xrightarrow{\beta^-\gamma} {}_{52}Te^{124}$	60d	0.48,0.65 2.37, others	1.7 others	Triple isomerism; beta counting. [24]
I^{131}	$_{53}I^{131} \xrightarrow[(1)\ 15\%]{\beta^-\gamma} {}_{54}Xe^{131}$; $\xrightarrow[(2)\ 85\%]{\beta^-\gamma}$	8.0d	(1) 0.315 (2) 0.65	0.63_8 0.36_4(79%) 0.28_4(6%) 0.08 (6%)	Beta counting. [3,7,24]
Ba^{140}	$_{56}Ba^{140} \xrightarrow[(A)]{\beta^-\gamma} {}_{57}La^{140} \xrightarrow[(B)]{\beta^-\gamma} {}_{58}Ce^{140}$	(A) 12.8d (B) 40.0h	0.4(25%) 1.05(75%) 0.90(20%) 1.40(70%) 2.1_2(10%)	0.5? 0.14 0.33_5(1%) 0.49(7%) 0.83(14%) 1.63(74%) 2.5(4%)	Beta counting after equilibrium has been reached between rate of decay of Ba^{140} and La^{140} [24]
W^{185}	$_{74}W^{185} \xrightarrow{\beta^-} {}_{75}Re^{185}$	74d	0.43	No	Beta counting similar to Ca^{45}
Au^{198}	$_{79}Au^{198} \xrightarrow{\beta^-\gamma} {}_{80}Hg^{198}$	2.69d	0.96	0.411_2	Beta counting; [24,25] Au^{198} should be best suited since it has a hard beta.
Hg^{203}	$_{80}Hg^{203} \xrightarrow{\beta^-,[\gamma],e^-} {}_{81}Tl^{203}$	48d	0.21	0.279 (~27% internally converted)	Beta counting similar to C^{14} and S^{35}. [25]
$^{t}Pb^{212}$	$_{82}Pb^{212} \xrightarrow{\beta^-\gamma}$ (A) $_{83}Bi^{212}$ $\xrightarrow{(B)_1\ \beta^-}$ $_{84}Po^{212}$ $\xrightarrow{\alpha}$ (C) $_{82}Pb^{208}$; $_{83}Bi^{212} \xrightarrow{(B)_2\ \alpha} {}_{81}Tl^{208} \xrightarrow{\beta^-\gamma,e^-}$ (D) $_{82}Pb^{208}$	(A) 10.6h (B) 60.5m (C) 2.6×10^{-7}s (D) 3.1m	0.19(5%) 0.36 (95%) $(B)_1$ (66.3%) 0.45(2%) 0.62(4%) 1.52(14%) 2.25(80%) $(B)_2$ (33.7%) 6.04_2(70%) 6.08_1(27%) others (3%) 8.77_6 1.7_2	Some 0.27_6,0.51 0.58_2,2.62	Beta counting. [24,33,40]
$^{t}Bi^{210}$	$_{83}Bi^{210} \xrightarrow[(A)]{\beta^-} {}_{84}Po^{210} \xrightarrow[(B)]{\alpha} {}_{82}Pb^{206}$	(A) 5.0d (B) 138.3d	1.17 5.29_8	No	Beta counting. [24]

and some previously adsorbed ion or complex? On the other hand, are we dealing with physical adsorption by means of van der Waals forces? The structure of the adsorbing entity is of importance: Is it a molecular ion, the undissociated polar molecule, or the hydrolyzed product?

In studying an adsorption system, determination of the partition of adsorbate between liquid and solid surface would seem to be the logical starting point. This means that we have to determine the amount of the substance adsorbed at various concentrations in the liquid phase. The literature on adsorption has voluminous references to adsorption isotherms for gas-solid systems, especially where physical adsorption is involved, but very few solid-liquid systems involving chemosorption have been investigated. For the purpose of determining such relationships in flotation systems, carbon 14 would naturally be favored as the tracer element, since carbon is an important constituent of all collectors.

To illustrate the value of adsorption-concentration charts, a specific flotation system will be discussed in some detail. It is well known that quartz is floated by fatty acids—e.g., oleic acid—in the presence of metallic cations, especially those of the alkaline earths. These cations are usually referred to as activators, since oleic acid is ineffective as a collector for quartz in their absence. This activating effect has been observed in soap flotation operations where quartz is present, but the reasons for this effect are not clearly understood.

The system quartz-oleic acid-barium chloride-water has recently been studied by Schuhmann and Prakash.[38] By means of a simple vacuum flotation technique, these investigators determined experimentally the effect of the three variables, barium ion concentration, hydrogen ion concentration, and total oleic acid addition at constant surface area of the solid phase and constant volume of the liquid phase, on the floatability of the quartz. This effect can be represented graphically on Cartesian coordinates as a curve which separates nonflotation conditions from flotation conditions. This curve therefore plots the condition within the system at which flotation will just be possible as a function of two of these variables for a predetermined value of the third variable.

Schuhmann and Prakash attempted to explain the effect of pH on quartz flotation in terms of the equilibria involved. Since it was possible to pass from a condition of flotation to one of nonflotation by either increasing the oleic acid concentration or by dropping the pH, the following adsorption reaction was proposed as the controlling factor:

$$\boxed{SiO_2}\text{—Ba—Ol} + H^+ + Ol^- \rightleftharpoons \boxed{SiO_2}\text{—H} + BaOl_2\ (\text{ppt.}) \qquad (1)$$

floatable quartz — nonfloatable quartz

Ol^- represents the oleate ion. This reaction assumes an exchange between the hydrogen ion and the barium-oleate radical on the surface.

An equilibrium constant can be written for this exchange based on the second law of thermodynamics,

$$K_A = \frac{a_{H\,(ads.)} \cdot a_{BaOl_2}}{a_{(BaOl)} \cdot a_{H^+\,(soln.)}} a_{Ol^-\,(soln.)} \qquad (2)$$

This can be simplified to

$$K'_A = \frac{a_{H\,(ads.)}}{a_{BaOl} \cdot 10^{-pH} \cdot C_{Ol^-}} \qquad (3)$$

on assuming that Henry's law for dilute solutions is applicable, and that the activity of the barium oleate is unity.

Using the hypothesis that for the system under investigation the ideal case of Langmuirian adsorption is closely approached, Schuhmann and Prakash put this expression in the form

$$\frac{N_H}{N_{BaOl}} = K'_A \cdot [H^+][Ol^-], \qquad (4)$$

where N_H and N_{BaOl} represent the number of the sites occupied by H^+ and by $(BaOl)^+$, respectively. It is furthermore assumed that there are no sites occupied by other ionic species and substantially no vacant sites.

Theoretical reasoning indicates that the ratio N_H/N_{BaOl} should be constant when flotation is just possible, so that if equation (4) represents the facts, the product of the concentration of the hydrogen ion and oleate ion should be constant for the Schuhmann-Prakash contact curves. Good checks were obtained.

The experimental conditions of the Schuhmann-Prakash work were such that the amount adsorbed could be neglected with respect to the amount left in solution. These investigators were therefore justified in placing the concentrations of barium ion and oleic acid in solution equal to the total amount added to the system. However, in order to verify the proposed equilibrium constant experimentally we have to know the amounts adsorbed at equilibrium. If the surface area of the quartz is known it is possible to calculate the total number of sites available for cation adsorption in accordance with the area covered by each site on a quartz surface.[16] It is then possible to determine the ratio N_H/N_{BaOl} from the amount adsorbed, when we assume with the authors that no other ions occupy surface sites.

A determination of the amount of barium adsorbed on quartz over a wide range of pH and concentration is being made at present at MIT, using Ba^{140} as the tracer element. The use of barium 140 as the tracer introduces a number of complications in the detection method, since this radionuclide does not decay directly to a stable nuclide but first to a daughter nuclide (La^{140}) which is unstable, too. The decay characteristics of La^{140} have to be considered in conducting the radioanalysis for Ba^{140} (Table 1). The possibility of the adsorption of lanthanum and the consequent effect on the adsorption of barium also have to be considered. Adsorption-concentration graphs for barium on quartz at various pH have been determined by this method. The data thus obtained are now being correlated

with the theoretical implications of the proposed ion-exchange adsorption of Schuhmann and Prakash.

The next step on the program is to determine if the soap ions are adsorbed on the quartz in the absence of the activator, and in conjunction with this investigation it is proposed to use radioactive (carbon 14) sodium laurate as the collector. This will be followed by experiments with both activator and collector, the radionuclides being in the collector or in both the collector and activator. It is believed that these adsorption-concentration graphs might furnish information on the extent of soap ion adsorption in the absence of barium and might help in deciding whether, in the system Ba^{++}-L^-(laurate ion)-quartz, the adsorbed complex on the surface has the composition BaL^+ as suggested by Gaudin and Rizo-Patron[16] and by Schuhmann and Prakash. Such graphs might also indicate if this complex is preformed in the solution and then adsorbed according to the reactions,

$$BaL_2 \rightleftharpoons BaL^+ + L^-$$
$$BaL^+_{(soln.)} \rightleftharpoons BaL^+_{(ads.)}$$

A number of flotation collecting agents belong to a group which has become known as "colloidal electrolytes." Soaps are examples. Other examples of colloidal electrolytes are the long-chained primary amine salts, alkali alkyl sulphonates, and sulphates and quaternary ammonium compounds. These colloidal electrolytes are useful for the treatment of nonmetallic minerals and metallic oxide ores. An understanding of the adsorption of these compounds is complicated by the fact that their thermodynamic equilibria in solution are complex and not completely understood, because of the hydrolysis of the salts, association of molecules, and micelle formation, even in very dilute solutions.

Adsorption isotherms have also been determined for the system dodecyl ammonium acetate-quartz over a wide range of concentration of this cationic-type colloidal electrolyte,[10, 13] using closely sized quartz fractions. Two experimental procedures were used. In one type a given amount of quartz was agitated for a constant period of time with a fixed volume of solution. The mineral was then separated from the liquor and the activity determined by the internal gas-counting method.[14] In the other approach a large volume of solution was passed through a short column of quartz and the analysis then made similarly to the previous method. The average size of quartz used in these two adsorption experiments was about 40 microns for the agitation and 20 microns for the column method. On plotting the logarithm of the amount adsorbed per unit area against the logarithm of the concentration in solution, it was found that the data obtained by the two methods fall on the same curve and that it is substantially a straight line of slope one half over a wide range of amine concentration. It is hoped that this slope of one half can be related to the chemical nature of the adsorbing entity and its equilibrium relationship in the solution phase.

Correlation of maximal adsorption with optimum recovery of quartz at various amounts of amine collector in batch flotation tests has also been made.[13] Contrary to the most generally accepted view that a mineral surface has to be covered with a monomolecular film of collector before flotation is possible, such a correlation has shown that at the inception of flotation only a very small fraction of the total surface is covered. For the near-complete flotation of quartz of an average size of 20 microns, only 4–5 per cent of the surface needs to be covered.

Cyanide ion has been shown to have an important effect on the selective flotation of many metallic sulphides. This ion is commonly known in flotation terminology as a depressing agent, since by its use it is possible to prevent the flotation of the sulphide mineral with a xanthate-type collector. The chemistry of cyanide compounds has been studied intensively, and it is generally recognized that one important function of the cyanide is the complexing of free metallic ions in solution, thereby decreasing indirectly the concentration of adsorbed metallic ions. There appears to be a relation between the concentration of metallic ions on the surface and the floatability of the sulfide comparable to the quartz-metallic ion-soap system discussed above. Whether the cyanide ion functions as a depressing agent solely by reaction in the liquid phase has not been established satisfactorily. To quote Wark: "Whether the cyanide ion itself is adsorbed instead of the xanthate has not been decided experimentally, because up to the present [1938] a method for the detection or estimation of such small concentrations of cyanide has not been discovered."[45] It seems that the tracer method may be used to resolve the dilemma. It should also be useful in further studies of the various equilibria involved in the sulfide-cyanide-metallic ion-xanthate system. Experiments along this line are planned for the immediate future at MIT.

The rate of adsorption is another important phase of adsorption studies in general and the flotation system in particular. Flotation in practice is a continuous operation, and the time of residence of a mineral particle in a cell may have an important bearing on the probability of its floating. This

time of residence may be less than the time required for equilibrium to be reached in the distribution of the collector between the mineral surface and the bulk of the liquid phase. Even though for a given amount of collector addition to the system it is not essential that an equilibrium be reached, the time of residence may still be shorter than the actual time required to reach that critical value of surface coverage which determines flotation or nonflotation of the particle. Rate of adsorption studies can then be correlated with the kinetics of flotation, where we also have to consider the adhesion probability and collision frequency of air bubbles with particles.[17, 37, 44]

Kinetic adsorption studies are under way by which it is proposed to determine the rate of adsorption of radioactive ethyl xanthate (S^{35}) on galena. It is expected that diffusion in the liquid across the relatively stagnant film surrounding the particles will be a large factor.

In addition to the rate-of-adsorption studies with collectors, similar investigations are being made on the rates of activation of mineral surfaces by metallic cations. The sphalerite-Cu^{64}, sphalerite-Ag^{110} systems have been chosen for investigation. A continuous radioanalysis of the progress of adsorption is being made by passing the radioactive solution at a constant rate through a column of sphalerite and by determining continuously the activity in the effluent.

So far in the discussion of the applicability of the tracer method to the flotation system we have chosen the various kinds of flotation reagents such as collectors, activators, and depressants as tracer compounds. In addition to these chemical components, the mineral itself is an important component of the heterogeneous system. The surface composition of the mineral surface is in dynamic equilibrium with the surrounding liquid medium and therefore also subject to compositional variation. Complex equilibria may be expected to exist at the mineral surface even in the absence of flotation reagents. Thus we should interest ourselves in the exchange of lattice atoms with ions of the same or related kinds in solution, in the rate of oxidation of such a surface, in the nature of the oxidation products, and in the zeta-potential of the surface as a flotation criterion. Some investigators believe that the flotation of base metal sulfides with xanthates is not possible unless the sulfur atoms at the surface are oxidized to $SO_4^=$ or other oxides of sulfur, and that xanthate adsorption is a stoichiometric exchange between these compounds and the xanthate ion. In order to verify or disprove such a "theory" and to study the rate of oxidation at a pyrite surface, radioactive pyrite has been synthesized from radioactive barium sulfide (S^{35}).

In conclusion it may be mentioned that, although this article has dealt with the use of tracers in helping build a sounder flotation theory, this by no means covers their possibilities in other mineral engineering problems. Aside from the use of tracers to establish flows in plants having complex flowsheets (as by using Na^{24}), they should prove useful in leaching, thickening, and filtration problems, as well as in a number of pyrometallurgical operations where self-diffusion becomes a factor of even greater interest than in mineral dressing operations.

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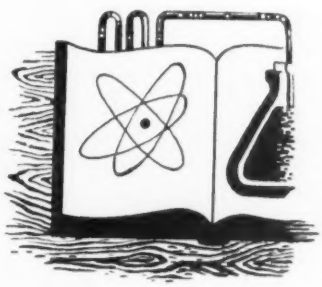

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Principles and Problems of Biological Growth

H. R. CRANE

The author, who is a Californian (Ph.D., Cal Tech, 1934), has been in the Department of Physics at the University of Michigan since 1935. He has also done work at MIT, the Carnegie Institution, with the Office of Scientific Research and Development, the Navy's Applied Physics Laboratory at Johns Hopkins, and the Manhattan Project. His principal field is nuclear physics.

THE problem of how viruses and other protein particles reproduce themselves, or are duplicated, forms one of the most fascinating fields in all science. Thousands of individual facts have been discovered about the chemical and physical changes that accompany the process; yet because of the enormous complexity of the systems that have to be dealt with, only the vaguest ideas of the mechanism as a whole are available. If one tries to recall the main theories that have characterized the speculation on the problem so far, four or five broad concepts come to mind. One, which is found frequently in the earlier literature, is that in which a mysterious "organizing" force, possibly residing in a "nucleus," is invoked, whose function is to arrange and rearrange the molecules, or building blocks, into the form of the final product. The concept calls to mind a submicroscopic demon with blueprint in hand, directing the traffic of the molecules, so that each assumes its proper place in the grand plan. Another, which is less vague, has been inspired by the observation of the beautiful and orderly way in which crystals grow. The fact that each layer added to a growing crystal of any simple chemical substance turns out to be an exact reproduction of the preceding layer has served as the basis for the idea that a large protein molecule, particularly a flat one, may cause an identical one to be built upon it by a process akin to crystalline growth. Another idea is growth by what we may call addition, or accretion; that is, the addition of one building unit after another, in such a way that each addition is determined by what has preceded it. Here, presumably, the right building block would add on at the right place, and only the right place, because of its highly specific properties. Still another broad concept which seems attractive is suggested by the recent ideas about the production of antibodies, in which it is thought that protein material may form on the surface of an antigen particle so as to produce a new particle whose contour is complementary to that of the original. Finally, an idea may be mentioned in which a protein particle, particularly a virus, does not reproduce itself at all in a direct sense, but stimulates the host cell to synthesize a copy of it.

Although no one of the above lines of approach can be said to be the correct one, it does seem to be possible to point out one feature which is common to all of them: that large protein particles are somehow assembled out of substituent material of a lower order of size; that is to say, merely, that they do not appear suddenly from nowhere but in some way grow or get put together out of smaller parts. From this point it would then seem to be safe to go just one step further and say: Since any number of particles of a given species, growing independently, can end up having exactly the same architecture, there must be some reason why each substituent part is able to find its very definite and proper place in the structure as it goes together. If we do not care to be so definite as to say that something closely related to the well-known property of "specificity" is responsible, then we may at least concede that a chemical or physical individuality of the building block is responsible—which is in a sense saying the same thing.

For some time I have felt that there are a number of interesting and simple physical principles which are pertinent to any discussion of growth in the broad sense as indicated above, and which, for that reason, might with profit be elucidated upon. One thing which should be held in mind is that what we shall discuss are for the most part fairly commonplace ideas, drawn from the physical sciences, and therefore not speculative, in themselves. The point at which speculation would enter the scene would be in the question of how far or how literally such principles might be applied to biological cases.

Many people have trouble understanding the

things that physicists say. I know because I am a physicist. So before going further let me make a few remarks which I hope will give the reader a better idea of what I shall try to accomplish and, more important, what I shall not try to accomplish. More often than not, when physicists and nonphysicists fail to find common ground, the specialized lingo and subject matter of physicists are given the blame. This is the first and most obvious reason. A less obvious but more basic reason lies not in the language at all, but in the peculiar method of reasoning or habit of thought which the physicist is trained to use and to accept. If you inquire of different kinds of investigators—physicists, biologists, sociologists, etc.—as to the ultimate goals they hope to attain when they attack a research problem, they will all give the same answer: they seek to "understand" whatever phenomenon is in question. But by "understanding" each may mean something quite different. Since my hope in writing this article is to help prepare the ground toward a better understanding of the phenomenon of growth, it is only fair that I try to explain what *I* mean by "understanding."

When a physicist sees a boulder rolling down a hillside he feels that he understands the phenomenon completely. There is nothing that excites his scientific curiosity, and he would consider it a waste of time to do research on the subject. Yet if he were given all the data he could desire—weight and shape of the boulder, contour of the hill, etc.—and asked to try to predict for you just where the boulder would come to rest, he would fail utterly. How, then, can he be so bold as to say that he understands it completely? A great many topics in physics are involved simultaneously in the rolling of the boulder: the law of falling bodies, conservation of momentum, conservation of energy, friction, elasticity, fluid dynamics—an almost endless list. Each of these topics will be found in a separate chapter in the physics textbook. Each is treated in pure, isolated form, and the example used in the book is invariably so chosen that just the principle to be illustrated comes into play and everything else is excluded. Acceleration due to gravity is treated by setting up the example of a ball dropping in a vacuum. The principle of the conservation of momentum is treated by using a hypothetical example of blocks of material sliding on a frictionless plane, and excluding any drag due to the air, etc. In fact, making examples which involve only a single principle at a time and in pure form sometimes leads one into the ridiculous, as in the classical example of the infinite avalanche held by a weightless string. Thus physics becomes a science based upon a thousand pigeonholes—each principle isolated and treated in pure form. (I am aware, of course, that physics is not alone in this.) So when the physicist says he understands the phenomenon of the boulder rolling down the hill he merely means that he understands all the pure ingredients that have been stirred together to comprise the phenomenon. Give him credit for a little more than this: he will not be satisfied unless he knows something about the proportions of the ingredients. He will want to know which principle or principles are predominant in controlling the motion of the boulder and which ones are of minor importance. But the point I want to bring out is that he will be reasonably happy and satisfied without requiring that he be able to combine all the principles into one grand equation giving in detail the motion of the particular boulder. The latter operation would come under the heading of complexity rather than principle.

Mathematics supplies all the implements for the complete solution of the boulder problem, but the sheer complexity of the equations would be such that it would be humanly impossible, or at least unprofitable, to carry the solution through in full detail. Natural phenomena are frightfully complex, and the physicist has grown up to be, in a sense, an opportunist. He has learned to live with complexity and is satisfied to say that he understands a great deal about a phenomenon if only he knows all the principles involved and something of the order of their importance in the particular case. Happily, in many practical cases, one or a very few principles predominate to such a great extent that fairly exact calculations become possible. For instance, when an athlete puts the shot, the motion of the shot can be treated with great accuracy using the simple laws of falling bodies alone, since air friction, the rotation of the earth, the attractive force of the sun and moon, and many other influences are comparatively very small; but to say flatly that an exact solution is possible would be erroneous. Physics has the just reputation of being an exact science, but the exactness lies only in the isolated principles. Nevertheless, the value and power of approaching the understanding of complex physical phenomena by the route I have described are attested by the abundance of real fruit it has borne.

To return to the discussion that is to follow, the reader will see more clearly what my objective is: it is to present some principles in "pure form" which should be helpful in the attempt to gain some understanding of the phenomenon of growth, using the word "understanding" in the special sense in which the physicist uses it. Nothing more is in-

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tended. I do not believe that, even assuming that we had at our disposal all the pure principles involved, it would be possible in the near future to combine them into any complete model or single "grand equation" which would describe the growth process. Complexity alone would stall any such attempt. Nevertheless, I am highly optimistic as to how far we may be able to go by just understanding clearly some of the isolated principles which pertain to a problem, when the problem is too complex to handle as a whole.

Some Properties of Chains

The simplest kind. I pointed out at the beginning that, under practically any conceivable picture of the growth process, small bits of matter must somehow get put together to form larger bits. A good many things can be said about the putting together of bits of anything, just from basic principles—things which apply equally well whether we are talking about protein particles, railroad cars, or almost anything else. Let us consider a large number of particles in a liquid medium, say, composed of just five different kinds, as shown in the upper part of Figure 1. (To a chemist a five-component system would be a complicated one; to a biologist a hopelessly oversimplified one!) Each particle has two ends, or spots, which have specific combining power, indicated by A', B; B', C; C', D; etc. A can attach only to A', B only to B', etc. Furthermore, when an $A'B$ unit attaches to a $B'C$ unit, the angles —that is, the orientation of the one with respect to the other—will be the same always. This is a direct consequence of the starting postulate that all the particles of a given species or kind are precisely like one another in every respect. Clearly, the assortment of building blocks which we have given can form a chain, as drawn in the lower part of Figure 1. The chain will be linearly periodic in structure, which is to say that as we move along the chain we find that the sequence of kinds of blocks repeats, and the angular relations between successive particles form a repeating pattern as well. (If the reader will turn ahead to the photographs of the matchbox models he will see examples of chains which satisfy the conditions just laid down.)

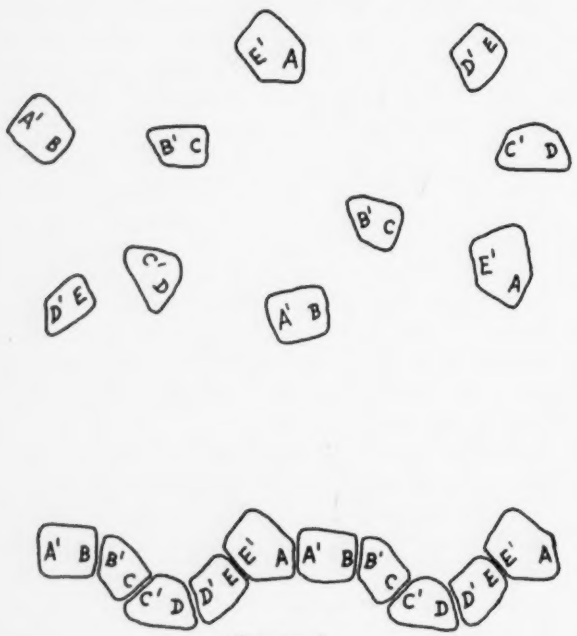


FIGURE 1.

Wherever A meets A', B meets B', etc., new chains of the same kind will start to grow spontaneously, and each one will grow at both its ends, until the supply of building units is depleted. The average length that the chains attain will depend upon the supply of building units and the number of chains competing for them. Some further properties of such a simple chain are the following: The fact that it is linearly periodic in structure means that it will be linearly periodic as to *all* its properties, such as mechanical strength, chemical or electrical character, as well as the geometrical form, which has been mentioned. If, for example, one kind of link in the chain happens to be weak mechanically (say the connection $C\,C'$ in the figure), the weak link will occur periodically along the chain, that is, at places separated from one another by the distance which may be called the *repetition length*. Therefore, if we were to break the chain up by stirring, we would expect it to come apart at the periodically occurring weak places, and would expect to find, not fragments of random lengths, but only fragments having lengths equal to the basic repetition length or multiples of it. Similarly, some link (say $D\,D'$) might be especially susceptible to splitting by acid; therefore treatment with acid would yield fragments equal to the repetition length or multiples of it. If, further, we consider two acid-susceptible links within each repetition length ($A\,A'$ and $D\,D'$), one of them more susceptible than the other, we would get, at one acid concentration a splitting into pieces equal to the repetition length, and at a higher concentration a further splitting into two different kinds of particles, whose lengths would add up to the repetition length. If we but imagine that this splitting by the acid is reversible, as it is in fact often found to be, we then see that we are dealing with a behavior familiar in chemistry—the change back and forth from one molecular weight to another or to a combination when the hydrogen ion concentration is changed.

If the liquid out of which the chain is to grow

contains N different kinds of building blocks, the maximum length to which the chain can grow before starting to repeat is that corresponding to N blocks. This is clearly so because in order *not* to repeat, at each addition, the added particle would have to be of a kind not already added. Therefore, provided an indefinitely repeating chain is possible at all, it will repeat after not more than N particles have been added. The formation of a repeating chain is, of course, not always possible, using a given assortment of kinds of building blocks. The example used and illustrated in Figure 1 was obviously set up so that the five kinds of particles supplied had just the right combining properties to be able to form a chain of five links. In a more realistic situation we might first of all find that, if a chain were possible, it would not make use of all the kinds of particles supplied. The repetition would still satisfy the rule given above, in that repetition would have to occur after not more than N particles had been added. We might find, second, that a kind of particle was present which would terminate the chain. To illustrate, let us suppose that we were to replace all the $A\ A'$ blocks with a kind of block Ax where x indicates a blank end which does not have the ability to attach to any of the other blocks present. Then there would be formed, instead of chains capable of unending growth, a large number of pieces of chain of definite and uniform length (Fig. 2). We could, instead of replacing all the $A\ A'$ blocks with Ax, introduce just a small concentration of Ax. This would act as a "poison" and would terminate only those chains which it happened to get built into. What I have just said is familiar to those versed in polymer chemistry.

Non-self-starting chains. One of the conspicuous properties of the kind of system I have discussed so far (a liquid containing an assortment of building blocks of the kinds shown in Figure 1) is that new chains can start spontaneously throughout the liquid as long as the supply of building blocks lasts. Although this kind of polymerization process undoubtedly has wide applications in living systems, one has the feeling that it does not lie very close to the heart of the problem of biological reproduction, just because of the self-starting feature. The most outstanding property of biological objects is just that they do *not* start spontaneously, but only duplicate what is already present. It will be interesting, then, to see if by some simple modifications we can conjure up a polymerization process that is not self-starting. It turns out to be rather easy. All that is necessary is to stipulate that the chemical (or physical) attachment of the particle being added requires, not just contact with the previous one, but

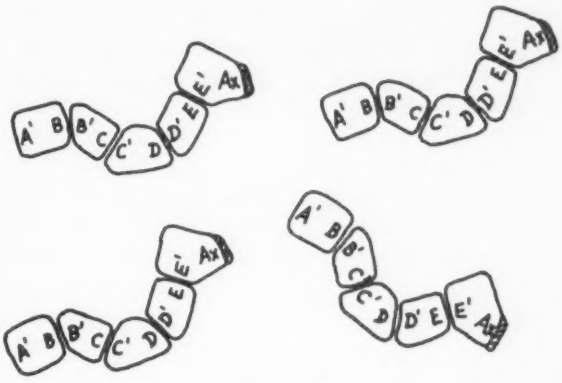


FIGURE 2.

with two or more at the same time. Such a stipulation actually goes hand in hand with the requirements for a high degree of specificity, as will be explained at a point further on.

The idea is indicated in simplified form (and in two dimensions)* in Figure 3, which involves several kinds of building blocks, just as in the first example. Since it is no longer a self-starting system, it must of course be assumed that some of the structure is already in existence. That is the shaded part. The others build on in the order indicated by their numbers. It can be seen that each one added makes connection to two of the blocks already there. The objection may be raised that blocks 1 and 2, for example, *might* happen to line up in the position shown for long enough for 3 to attach, which would allow the chain to start by itself. So they might, in the case of the simple chain shown, but if the reader has a vivid imagination he can easily construct a somewhat more complex system in which the prob-

* In the diagrams and examples used it has been necessary, for reasons of simplicity, to deal with linear, or at best two-dimensional, arrangements of particles. As far as one can foresee, the extension of the same principles to three-dimensional arrangements could be done without raising essential difficulties, but it would of course greatly increase the complexity. For example, there is no apparent reason why the arrangement of Figure 3 could not be shown as having a depth, as well as a width, of many particles. The extension of such ideas to three dimensions would require careful consideration, and would be complicated, but it would be interesting to try, because three-dimensional arrangements would be more directly comparable with actual biological material.

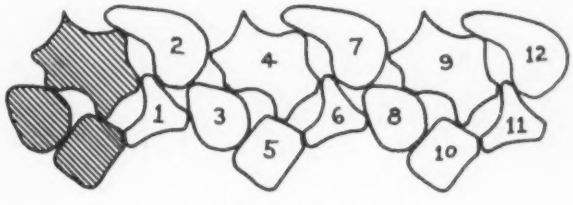


FIGURE 3.

ability of self-starting will be negligible. In any event, it is only the principle that I hope to illustrate: the reader can extend it as he wishes. As an interesting exercise I have devised a non-self-starting system out of only two varieties of building units (Fig. 4). A system obeying the rules just postulated would behave in an interesting way: first, no chains would form spontaneously in the liquid containing the building blocks. If a piece of such a chain were put into the liquid as a seed, however, a single long chain would grow, eventually exhausting the supply of blocks. If the chain were for any reason broken during the process, each fragment would then serve as the seed for a new chain. It is not hard to imagine ways, without departing from perfectly sound physics and chemistry, in which such chains could break, periodically, that is, at the same place in each cycle of structure. If this were the case, then we would have an artificial system which would be a good imitation of certain biological systems: one seed would produce a large number of copies of itself. There are some important points in regard to possible physical and chemical processes by which a structure of this kind can break at the same time it is growing, and a discussion of these will next be undertaken.

Periodic breaking. It should be kept foremost in mind that, no matter what the mechanism is that causes breaking, the tendency will be for the chain to break periodically—that is, at places separated by the repetition length—just as was pointed out in regard to the simpler kind of chain, because the mechanical strength, as well as all other properties, will be repetitive. The emphasis now should be upon ways in which periodic breaking may occur automatically, without outside help such as stirring. Stirring should not be excluded entirely, however, since fluid motion does occur in living systems; therefore let us put down stirring as the first of the suggested mechanisms and leave it at that. The second mechanism I want to call to mind has to do with what an engineer would call "stresses in an overdetermined structure." In an overdetermined steel structure, it is a common observation that if a girder is welded in at one place, the new stresses and strains transmitted throughout the structure

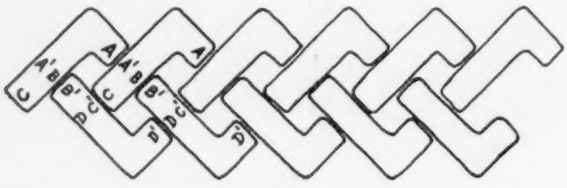
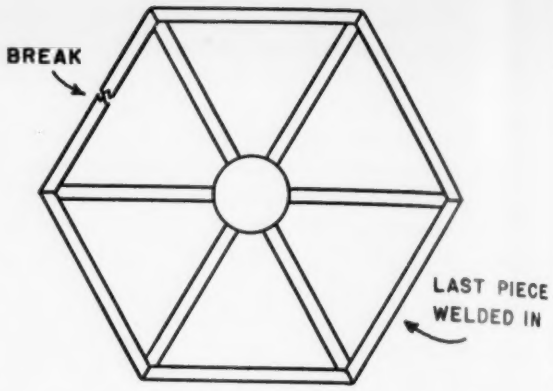


FIGURE 4.



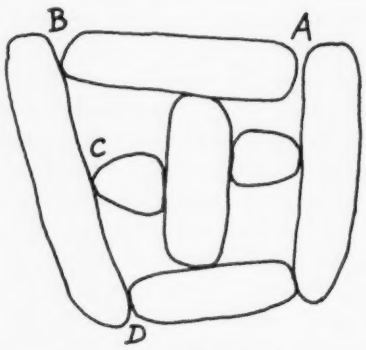


FIGURE 5.

may cause a joint quite far from the new girder to crack open. A good example would be the hexagon with spokes, as shown in the upper half of Figure 5. If, after the last side is welded in place, it shrinks slightly due to cooling, tension will be created in all the six sides, not just in the ones near the last weld. The resulting break may therefore just as well be at the farthest place from it as at any other place. Application of the principle to the non-self-starting structure of the type just discussed is not hard to imagine. A piece of such a structure is drawn in the lower part of Figure 5. If the last block on the right does not quite fit, the pulling together of joint *A* will create stresses throughout the structure. If joint *B* happens to be a somewhat weak one, the stresses might cause it to open, which might lead to a separation of the whole structure along the line through *B, C,* and *D*. Now if the reader's confidence has not been shaken by the number of assumptions made in this highly artificial example, he will see that a perfectly valid principle lies at the bottom of it—in fact, a principle that has cost many a structural engineer a night's sleep. During the war a story made the rounds about a ship whose sheet-iron hull and internal bracing had been welded together, with the gradual accumulation of stresses

due to its being a highly overdetermined structure. Just as the last welder finished the last joint and turned off his torch the ship cracked across the middle and fell into two pieces. Whether or not the story is 100 per cent true, there is nothing wrong with it in principle.

Third, a process may be mentioned which depends upon the *dynamic* nature of the processes involved. Anyone will readily agree that his body is being torn down at the same time that building processes are at work (with, he hopes, not too great an advantage on the side of the former). But the idea of structures on the molecular, or small-particle, scale of sizes suffering tearing down while they are being built is perhaps not quite so familiar. As a specific instance, suppose that in the structure in Figure 3 one or more of the blocks, say, types 4 and 7, are short-lived ones; that after they are produced, perhaps at a remote place, they are destroyed in a short length of time by the fluid in which they now find themselves. They are not destroyed so quickly, however, that they are not able to build into the chain in Figure 3 and remain intact for long enough to permit the building process to proceed safely beyond that point. Later, as their disintegration progresses, the chain comes apart at that point. It is not new, either in the realm of molecules or in our everyday world, to find that individuals serve a specific function in a larger process while they themselves are on their way to destruction.

A variation of the principle, which allows the same result to be accomplished, but which belongs more especially to the realm of very small particles or molecules, is based upon the laws of chance. Let us consider any fluid which contains a rather small number of molecules of a certain species. In a large sample of the fluid the concentration of that particular substance remains quite constant. If, however, we were to fix our attention upon a very small region in the fluid, say, a cubic micron, we would observe great changes in concentration from moment to moment. The particles, or molecules, of the material in question are in constant motion, and just on the basis of the laws of chance the number to be found within the particular cubic micron will be zero at some times, many at other times. Thus, if the scale is made small enough, one finds that the concentration of everything varies from moment to moment and from place to place. For this reason reactions can take place and combinations can be formed temporarily which would not be permitted if the average conditions were to hold true at all places at all times. For the growth of the non-self-starting chain we have been discussing, temporary combinations are enough to serve the purpose, which is to allow the building of the chain to progress through that point, and later to come apart again, behind the point of building. I have only been trying to rephrase a principle which is well known in much of chemistry, particularly catalytic chemistry.

A fourth mechanism of breaking has to do with the effect which mechanical distortion has upon the chemical reactivity of molecules. In many of the reactions of catalytic chemistry, two molecules which do not ordinarily react with each other will do so readily when one of them is distorted mechanically—simply bent or stretched. The distortion is often accomplished by temporary attachment to an intermediary which may be a surface or another molecule. A strikingly simple example of a change in reactivity due to molecular distortion is observed with a rubber band. Everyone knows that a rubber band that is exposed to the atmosphere in a highly stretched condition will disintegrate rapidly, whereas an identical one in a relaxed condition will be practically unaffected. It has already been suggested that stresses and strains in a structure of the kind we have been discussing may develop due to its "overdetermined" nature. These strains can affect the chemical reactivity of a building block after it has been built into a structure. Thus this gives a further possible way in which a chain may break at periodic intervals with "delayed action," so to speak.

The form of chains. The basic characteristic of the building block which was postulated in the beginning was that all blocks of a given species were identical with one another in every respect. A direct consequence of this was that whenever a block of one kind (say, *A*) attached to a block of another kind (say, *B*), it did so in exactly the same way—that is, Block *B* would occupy the same spatial orientation with respect to *A*. By extending this behavior to *C, D, E,* etc., we saw that repetition in structure would have to occur, and noted a number of other logical consequences. I have undertaken to carry out a little laboratory experiment on a scale which will allow us to see with our eyes what geometrical forms may be taken by chains which are built according to the rules of the game. In assembling the materials it was desirable to avoid choosing objects with a plane or an axis of symmetry, since that would provide more than one way for the attachment between a given pair. Furthermore, whatever objects were chosen had to be available in quantity, all identical. This eliminated such things as potatoes, which would have been ideal from the standpoint of irregularity. Somewhat in despair I settled upon using matchboxes for

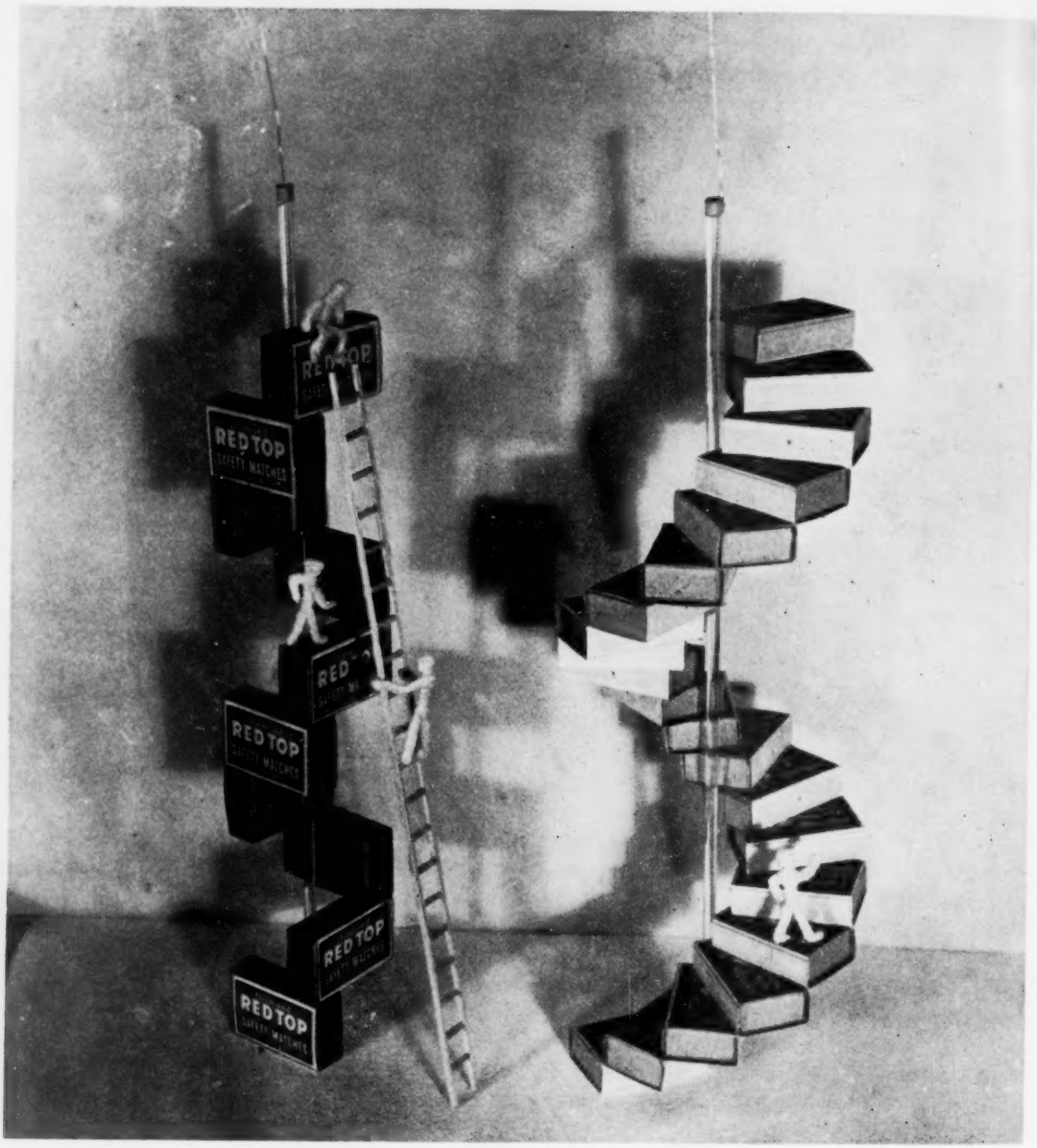


FIGURE 6.

most of the work. They are too regular in shape, having rectangular sides, but symmetry is absent if one takes into consideration the printed matter on their tops and bottoms. In assembling the models only one rule was observed: The attachment of one block to another was always done in exactly the same way, geometrically. All but one of the models were made with a single kind of building block, since results to be remarked upon could be seen more clearly in that way.

The first and most striking thing to be noted in the models is that all of them take the form of a screw, or helix, which winds around a *straight* axis. This is not only true of the models that are shown, but would be true of every chain put together according to the single rule I have mentioned.

Now let us inquire into the question of repetition in structure along the chain. Looking at the left-hand model in Figure 6, there are two ways in which a midget explorer might investigate the properties of the chain. He might climb up the chain, going from one box to the next, as indicated by the pipe-cleaner man standing on one of the boxes. Such an explorer would find that the structure repeats at every box. That is, if he were to stand on one box and take a good look at the details

of the chain from that vantage point, then move to the corresponding place on the next box and do the same, he would see not the slightest difference—(that is, assuming that he kept his attention strictly on the chain, and did not look at neighboring objects and did not carry his Boy Scout compass, because of course he faces in a new direction relative to the outside world every time he moves from one box to the next). The verdict of this explorer would be, then, that the repetition length in the chain is the distance from one box to the next. Quite a different answer would be given by the man who is climbing the ladder. The structure does not seem to him to repeat exactly until he has moved a distance corresponding to four boxes. Wherein lies the difference? Clearly in the fact that the second observer continued to face in the same direction, whereas the first one took a position which was always the same relative to the particular box he was standing on. Both are right, but their observations of the repetition length (measured along the axis) differ by a factor of 4. I am going to add that it was only due to my advance planning that the man on the ladder found any repetition at all. In cementing the boxes together in the first model, I intentionally made the angle between each box and the next exactly 90 degrees, so of course after moving along by four boxes, the direction comes back parallel to the original direction. In the second model (right side of Fig. 1) the angle through which the climber turns in moving from one box to the next is nothing simple like 90 degrees. Therefore, while the climber sees exact repetition in structure every time he advances from one box to the next, the man on the ladder may have to climb a very long way before he finds a box which has exactly the same orientation in space as the first one he noted. In fact, we could easily have chosen an angle between adjacent boxes which would prevent it from ever returning to the same orientation—such as $\sqrt{2}$ degrees.

To sum up what I have just tried to say: Every structure that is built according to the one rule laid down will be repetitive along a *screw axis,* which is to say it will be repetitive as seen by an observer who rotates around the axis as he moves upward along it, whether or not it is repetitive as seen by an observer who does not rotate. If the structure is now broken up into pieces which are equal to the repetition length or a multiple of it, all the pieces will be identical to one another.

Let me make a remark here about the ordinary three-dimensional crystal. This is a structure which displays repetition in three different directions. If we were to make a large number of chains like the left-hand one in Figure 6 and were to stand them in uniformly spaced rows and columns on a table top (like a well-laid-out orchard), we would have a solid which would exhibit exact repetition to an observer moving (1) vertically, (2) along a line parallel to the direction of the rows, or (3) along a line parallel to the columns. The same can be done with any chain which the man on the ladder finds to be repetitive. It cannot be done with a chain which is repetitive only along a screw axis. In simple substances of relatively simple structure, such as the chemist deals with, the molecules can readily arrange themselves so that they satisfy the requirements for crystal formation. But, in the case of large particles of matter, we expect the likelihood of chains which are repetitive to the man on the ladder to be small, and therefore we expect to find more often structures which are repetitive along the screw axis but not in three directions, as in a crystal.

A practical lesson to be learned from the foregoing discussion is that any structure which is straight or rodlike when seen at "low magnification" is probably a structure having repetition along a screw axis. At somewhat greater magnification such a structure would be expected to reveal a helical, zigzag, or banded appearance. To imagine a straight structure which did not owe its straightness to the cause just given would require the assumption of a highly fortuitous combination of angles between successive blocks.

One important qualification or addition to the above should be made; that is that if the core or backbone of a structure consists of a chain satisfying the above requirements, it should not be at all impossible to have side structures on it which occur at random, or nonperiodically, without destroying the geometrical properties of the backbone. On the molecular scale of sizes, the example which immediately comes to mind is the carbon chain backbone with side groups attached. The angles between neighboring carbons are so rigidly determined that the side groups do not much influence the geometry of the core.

In Figure 7 the left and middle models serve to emphasize further the variation in appearance which is gotten by different choices of the angle used between the successive boxes. The right-hand model shows that the same repetition along a screw axis is obtained by using several kinds of building blocks in rotation. But in the matchbox models just what one chooses to say that one box represents is quite flexible: one box may be taken to represent a short piece of chain consisting of several smaller units, all of these subgroups being identical to one another. In that way the matchbox chains really

embrace all that can be learned from the more complicated-looking one made of the balls and batteries, etc. There is really only a single principle involved.

Results of combining some of the principles discussed. In the foregoing paragraphs a number of principles were gathered together which pertained to the combining of small particles to form larger ones. The particular items touched upon were: periodicity, termination, automatic breaking, the non-self-starting feature, and geometrical form. Not all the kinds of behavior mentioned need apply to a system at once. It is interesting to see what happens if we select certain combinations of these,

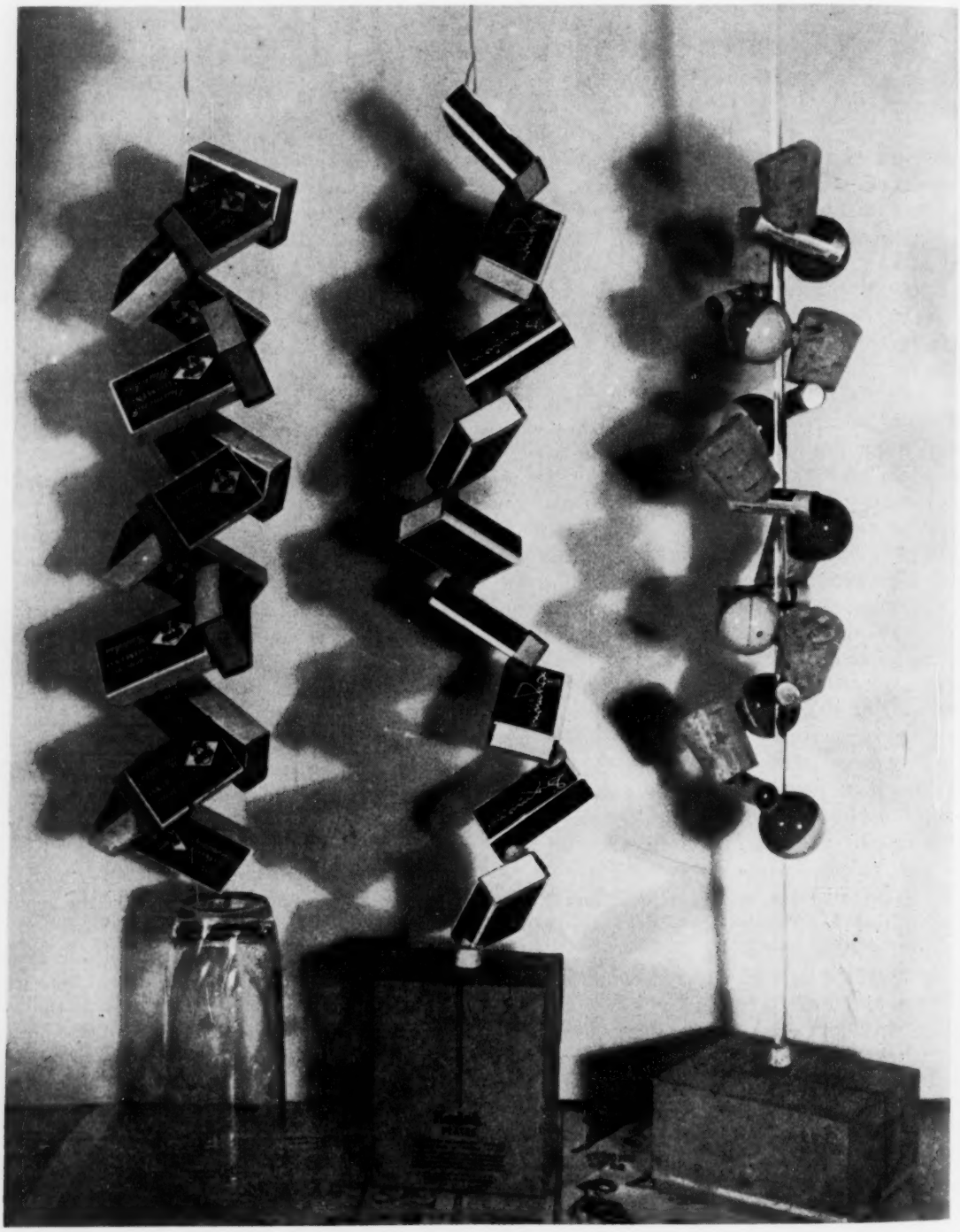


FIGURE 7.

since the result in each case shows at least a superficial resemblance to something we are familiar with in biology. The non-self-starting property is assumed.

1. Growth of chains without termination and without automatic breaking. Result: long, straight rods, but under higher magnification seen to be zigzag, helical, striated, etc., with straight axes. If broken up due to stirring effects, fragments would be expected to be multiples of a fundamental length, due to the fact that weak places are located periodically.

2. Termination, but with automatic breaking. Consider the structure after it has gone to termination (Fig. 8). A break occurs somewhere, setting free a fragment. The surface exposed grows again to termination. Breaking occurs again and a second fragment goes free, and so on—fragments are *produced* continually, but there is no *reproduction* either by the fragments (which are terminated) or by the producing structure. The fragment cannot be produced without the presence of the original structure. If we imagine that such a producing structure were a part of a cell wall or a nucleus, for example, the system would simulate the synthesis of particles by living material.

3. Continued growth (not terminated), with one automatic splitting per period. This case allows the unlimited multiplication of a "seed structure." Although splitting is presumed to occur at a constant distance from the growing end, so that all fragments at the instant of splitting off have the same standard size, one would not expect to find a simple uniformity in the population, since the fragments can continue to grow after splitting off. The size distribution can be predicted from the rates of growth at various stages of development, just as in any population.

4. Growth with more than one breakoff per period. This would give rise to two sizes of seeds at the instant of breakoff, but each would be capable of instituting the same chain of building.

5. Termination by poison. If, in any of the above situations, a "poison" were introduced which would terminate the growing structure at such a stage that it would not undergo breaking, the whole process would be brought to a halt. If poison were introduced into the situation of 3 or 4 so that termination was produced but breaking was not stopped, these would be transformed into the situation of 2.

Specificity or Selectivity

Physical and chemical basis. Much use was made in the foregoing paragraphs of the idea that one building block may be able to attach only to a

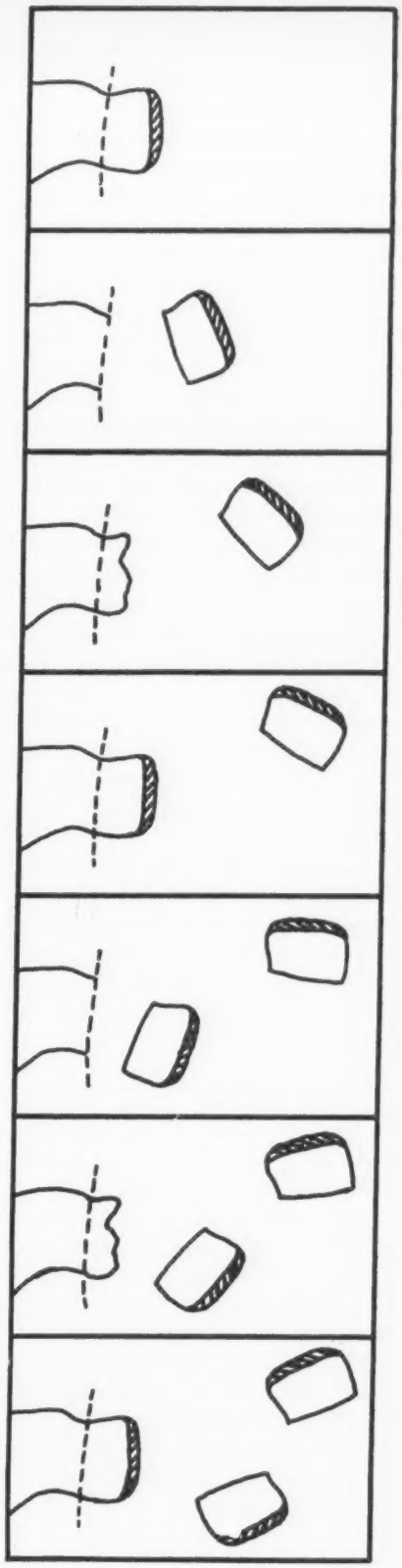


FIGURE 8.

particular other block or grouping of blocks. That, of course, is the general property that is closely related to what is called specificity, if it is not identical with it. I am tempted, however, to avoid using the term specificity and use in its stead selectivity because the former connotes so strongly the antigen antibody reaction and nothing else. There are a few interesting points to make about specificity, and in order to do so let us first review briefly the trend of present thought on the subject. The details of the mechanism by which such high selectivity is accomplished in any given case are a mystery, but the general rough outlines of the mechanism are not nowadays considered to be baffling at all. This happens to be one of the rare cases in which, chemically and physically speaking, the possibilities can immediately be narrowed down to relatively few. First, it may effectively be argued that the specific attachment of two particles is not accomplished through a single point of contact or single chemical bond-like connection, on the grounds that there would simply not be enough variety in such single connections to furnish the extreme individuality observed. The number of chemical elements and radicals that could be involved in simple combinations can almost be counted on one's fingers. One particle might have an NH radical which would act as a combining point; another a CO, etc.; but it is easy to see that to try to use those to give combining characteristics to particles so individualistic that one kind will be able to select one other kind to combine with out of perhaps hundreds of thousands of possibilities would present a problem indeed. To obtain the necessary variety one is compelled, then, to consider multiple contacts or connections, acting in cooperation in holding two particles together. As a matter of fact, the very old idea of the lock and key does not miss the mark very far. Without going to the extreme of drawing a picture approaching in appearance a Yale lock and key, as writers have sometimes been tempted to do, we might visualize the combination between two particles as in the upper part of Figure 9. Here a triple connection is shown. Not only must A' have an attraction for A, B' for B, and C' for C, but these combining spots must be located geometrically so that the configuration A, B, C is the spatial complement of A', B', C'. This at once multiplies the number of possible species of particle enormously. In order for such a scheme to give rise to extremely high selectivity one further condition must be supposed to hold, namely, that the connections are individually so weak that *all* of them acting in cooperation are necessary in order to hold the two particles together. Otherwise, for instance, if the AA' link were strong enough to hold by itself, the selectivity would be lost because the exact geometrical arrangement and exact nature of the other points of attachment would no longer be of prime importance. Thus we arrive at the conclusion that for a high degree of specificity the contact or combining spots on the two particles must be *multiple and weak*. Furthermore, those on one particle must have a geometrical arrangement which is complementary to the arrangement of those on the other, as is clear if all the spots are to be able to get near each other simultaneously. Opposite points must also, of course, attract each other.

Now let us look more closely at the nature of the chemical attachment between A and A', B and B' etc., in view of the virtually inescapable argument that the attachments must be weak. Chemical bonds such as the peptide linkage or any other electron-sharing combinations would probably be too strong —one or a very few of them would hold the particles together and therefore not give the extreme selectivity desired. The hydrogen bond is weaker and may be involved. The weakest kind of attraction is the van der Waals force, which acts between practically any atom or molecule and any other. It is the weakest and the most unselective. It would seem to be something of a contradiction if this, the least selective kind of attraction, were to play an important role in specificity, which is fantastically selective, but such may be the case. Its weakness is in its favor; the selectivity then would arise entirely from the geometrical complementarity of the contacting surfaces on the two particles, as indicated by the sketch in the lower part of Figure 9. In effect we would have *very many, very weak* combinations, which is just what is needed for high specificity. As to the details regarding the degree to which specificity is due to the extremely weak attractions just discussed and how much to somewhat stronger ones—the hydrogen bond, for instance—no one can say, but at least the general principles discussed seem to supply all the ingredients necessary for explaining the extremely great selectivity exhibited by particles in attaching themeslves to others. In any given case the truth probably lies somewhere between the two extremes shown in Figure 9.

By way of a simple illustration of how a multiplicity of weak attachments can increase selectivity, let us take an "academic" example. Let us assume that each of the particles has three contact spots, lined up opposite each other, just as they are in the upper part of Figure 9, and that there are 10 possible kinds of spots, $A, A', B, B', C, C', D, D', E$ and E', which attract each other only in pairs, such as

AA' or BB'. There are 1,000 possible combinations of spots for each particle. A particle which has A, C', D can attach only to an A', C, D' particle. The latter is only one combination out of 1,000 possible ones. If single point contacts had been used, each particle could attach to one out of 10 other kinds. Notice that this leaves out differences in geometrical configuration—if that element were added, the number of possibilities in the first example would be increased by another large factor.

Quantitative requirements. Having laid this much groundwork, let me now proceed to some remarks about the relation of the kind of selectivity we have been talking about to the formation of chains or structures. The first concerns the degree of selectivity required in the building of a structure of a given size out of parts of a lower order of size. As before, we can make an attempt only if we use a simple and idealized example, but nevertheless I believe that some good can be gotten out of it in illustrating a principle. Let us imagine, along lines already discussed, that a certain species of structure adds to itself 100 building blocks of various kinds by any process in which the blocks take their places one after another, and that after the 100 blocks have joined the structure it breaks so that there are then two structures capable of growth. The addition of the 100 building blocks constitutes, let us say, one reproduction cycle. Let us further imagine that if any one building block makes a mistake, that is, fails to build in at the right place, the whole structure will be spoiled so that it will not form two new ones. Therefore we get either two or none, depending upon whether a mistake occurs or not. With these simple, and admittedly arbitrary, rules we can at least arrive at some concrete consequences that will have some further interest for us. For example, it is clear that in order to have a population of particles which will just hold its own the chance for the occurrence of a mistake in a given cycle must be 50 per cent. If the chance is less than 50 per cent the population will increase, and if greater it will decrease. This means that to give a stationary population, the occurrence of a mistake at the addition of *each of the 100 building blocks* must be $\sqrt[100]{0.5}$.

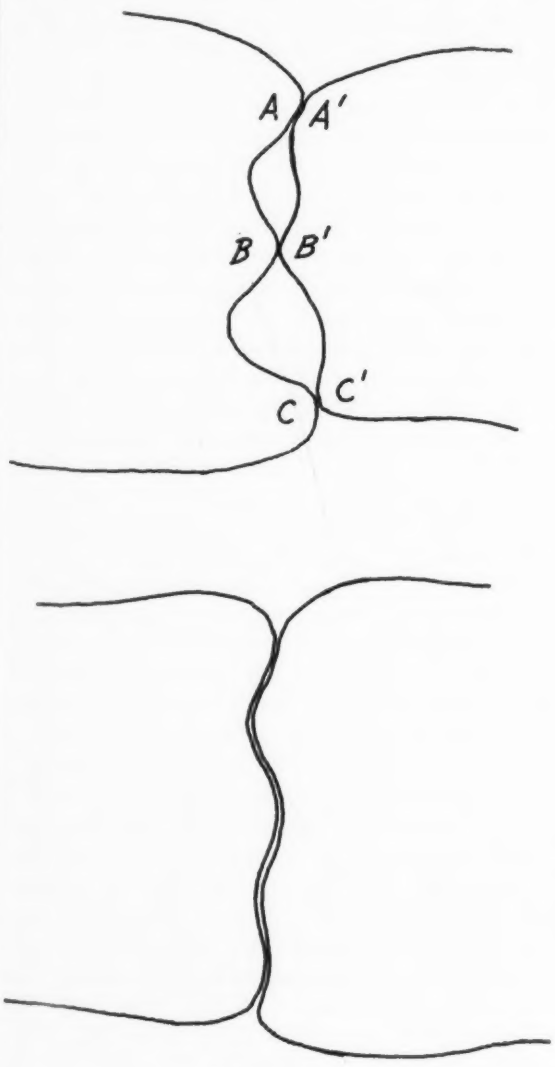


FIGURE 9.

Let us now define the degree of selectivity in the addition of an individual building block as the chance that a mistake will *not* be made. Thus 1.0 would indicate perfect selectivity, and 0.99 would indicate a 1 per cent chance for the occurrence of a mistake. In the above case the selectivity which we found to be required in the attachment of the individual block would be $1 - \sqrt[100]{0.5}$. Since this represents the balance point between growth and decline of the population, we might speak of it as the "critical selectivity." The critical selectivity is given by the equation $S^{\frac{W}{w}} = 0.5$ where W is the weight of the new particle at the time it breaks off and w is the weight of a building block. Now a particle of the same size might be constructed of a large number of small blocks or a small number of large blocks, and the degree of selectivity required in the two cases would be very much different. This is expressed in the graph in Figure 10, which has been worked out for the case in which the population just holds its own; that is, the chance of a mistake in each reproduction cycle is 50 per cent. The four lines represent particles of four different weights made up out of building blocks having weights given by the abscissa scale. The ordinates give the degree of specificity required in each case. For ex-

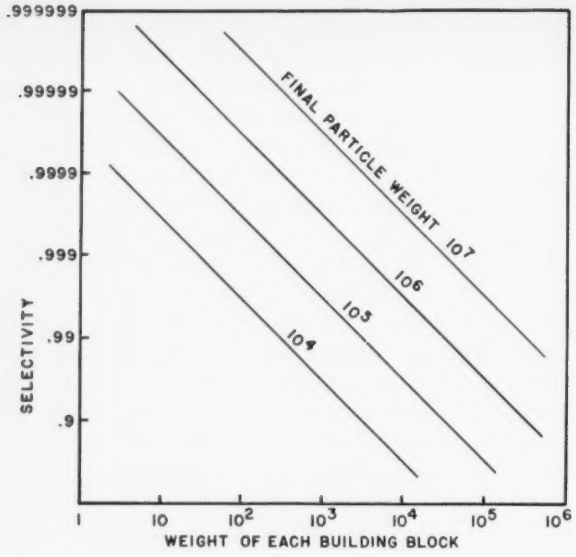


FIGURE 10.

ample, if a particle of weight 10^6 is made up of building blocks of weight 10^4, the ordinate scale says that the required selectivity in the addition of each block must be about 0.993 or greater for survival of the population.

I can almost hear my patient reader protesting that in any realistic case the various species of building blocks that combine to make up the bigger particle would not all be of the same size, that they would not all have the same selectivity, that they would not all play roles of equal importance, and that, anyway, a structure could probably stand a few mistakes and still survive. But what has just been developed is intended to be only a means of illustrating a principle.

We take a suggestion from the modern factory. The modern art of the mass production of automobiles, houses, radios, and other articles by the technique of using *subassemblies* may seem at first sight to be quite far removed from the question of the assembly of protein particles, but I believe it can be shown that some of the features are as applicable to one field as to the other. The production of an automobile by modern methods may be divided into different levels of assembly. The elementary materials, such as wire, bolts, nuts, etc., are produced in one or more factories; in other factories these are converted into subassemblies such as generators, dashboard instruments, etc.; in still other factories the latter converge to be made into motors and bodies; and so on. Two significant advantages of the subassembly method stand out: The culling out of defective units can be done at each level, so that a defective unit produced at one level does not get built into a larger unit and so make it defective in turn; more workmen can be working simultaneously on the subassemblies for a single automobile than could possibly crowd around that automobile if they were at work on it directly, without benefit of subassemblies. So both accuracy and speed are greatly facilitated.

Let us now think of the assembly of a large particle in terms of the subassembly process, this time leaving out of consideration the question of reproduction. (The particle we make here might later enter into a reproduction cycle.) Suppose that a particle is built up by a subassembly method, involving several stages. Let us say that we start with 1,000,000 small units, whose weights are designated as 1. The first subassembly operation consists of assembling these into groups of 10, so that we have 100,000 particles of the second level of size. (Some of these are going to be imperfect specimens, but let us wait a moment to talk about that.) The particles on the second size level then get put together in groups of 10 to give still larger ones, and so on. On the fourth and final level, each of the particles contains 1,000 of the original ones.

We are now in position to inquire as to the degree of selectivity required in such a subassembly operation, which is the question I have been leading up to. To be able to carry an example through in quantitative form, it will be necessary to make the rules quite definite, by assuming: (*a*) that a unit which acquires a "mistake" in structure at one subassembly level will be incapable of entering into the succeeding subassembly operation, but that it will consume its full quota of 10 building blocks in the level in which the mistake occurs; and (*b*) the degree of selectivity, as defined earlier, is the same at all levels. We shall take it to be 0.990.

Using the rules just given, a diagram has been constructed which shows what the fate of the initial 1,000,000 building blocks will be. The chance of a mistake occurring at each addition is 1–0.990, or 1 per cent. Therefore the chance that a mistake in structure will exist after 10 have gone together will be $1-(0.990)^{10}$, or 9.55 per cent. Referring to the diagram, it can be seen that the outcome of the first subassembly operation was 90,450 perfect specimens and 9,550 imperfect ones. In the next subassembly the same percentage of imperfect specimens will result, and so on. The over-all score is 740 out of a possible 1,000. Now let us see what the score would have been if we had not made use of subassemblies, but if we had built each of the final particles by adding 1,000 of the small blocks individually. The same rules and the same basic selectivity for the individual addition are assumed.

The lower part of Figure 11 shows the result: only one perfect particle out of a possible 1,000.*

The foregoing comparison, even though it had to be done with quite arbitrary "rules of play," at least serves to suggest intriguing possibilities for the application of the subassembly principle, in processes quite different from the manufacture of automobiles. If a biological principle is suggested by the example of Figure 11, it is that of getting the most out of a given degree of selectivity.

A further result of the subassembly principle has to do with the speed with which a complicated structure can be made. It is not so easy to make a simplified example of this without adopting rules that would seem excessively artificial, and perhaps it would be wiser not to attempt it. One might make the following remark, however. If we should assume that, in the example given previously, each growing unit were to accumulate building blocks at the average rate of one per second, the time for the entire process shown in the upper part of Figure 11 to be completed would be 30 seconds. If the growing unit in the process shown in the lower part of the figure were to accumulate building blocks at the same average rate, the time required for completion would be 1,000 seconds. As I have already indicated, this is a somewhat strained example, but I believe the basic idea will be clear enough.

In conclusion, let me tell a little story. During the early part of the war I had occasion to go through a vacuum tube factory, where the miniature tubes for the proximity fuse were being made. There I saw rows of women assembling the most minute parts under magnifying glasses, and giving every appearance of being highly skilled jewelers. I asked if the training of these women did not present a great problem. To my surprise my guide told me that a completely green person could be put on the production line four hours after walking in the door. "Of course," he said, "they make mistakes, but each worker does only a small thing, and the pieces he spoils are, of course, not used in the next subassembly operation." Here a highly complex and precise product was being made entirely by unskilled hands, not only by virtue of the fact that each worker had only a small job to learn to do, but also by virtue of the fact that the system provided for casting aside the imperfect units at many levels of assembly. Therefore no individual was required to make a high score on accuracy. It was at this time that I was struck by the apparent parallels that existed between this method of factory production and many processes in nature—from evolution and natural selection down to the relations of the smallest molecules. The problem of how the fantastically complex and precise "products" could be manufactured in nature without the requirement of an excessively high degree of selectivity or accuracy at any one stage of development seemed slightly more understandable.

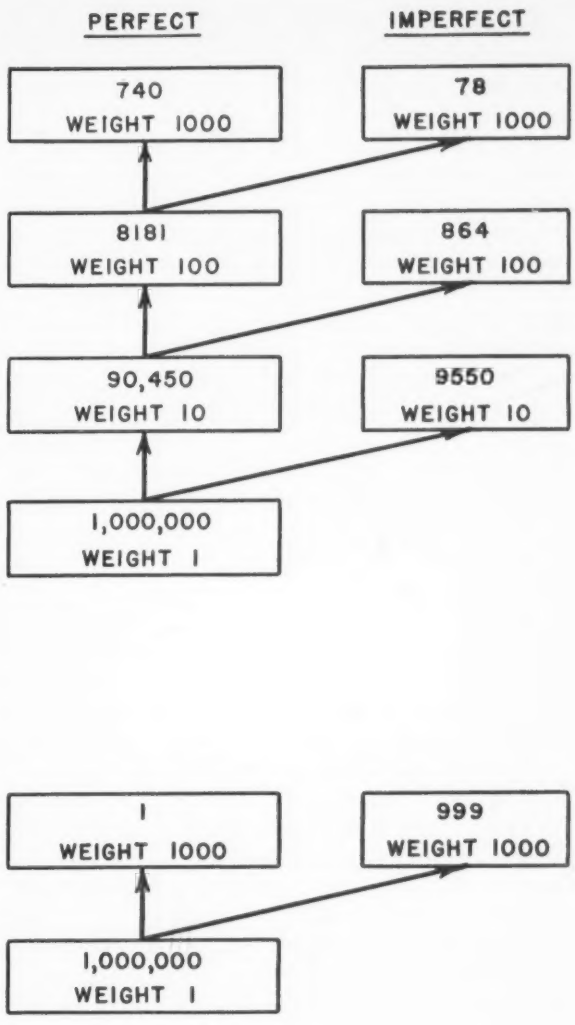


FIGURE 11.

* The chance that 1,000 building blocks can combine together without the occurrence of a mistake is found by raising the individual selectivity, 0.99, to the one-thousandth power. The result is 0.00004. Since 1,000 final particles are formed, the chance that there will be *one* perfect one is $1{,}000 \times 0.00004$, or 0.04. In presenting the idea in the above text, I did not want to add to the confusion by dealing with fractions of particles, so I said that we would obtain one perfect particle. Actually, there is only a 4 per cent chance of getting one. Using this more precise estimate makes the contrast between the upper and the lower parts of Figure 11 even more striking.

The Current Five-Year Plan of Soviet Science in Historical Perspective

JOSEF BROŽEK

A native of Czechoslovakia, Dr. Brožek (Ph.D., Charles, 1937) came to the United States in 1939. In 1941 he became associated with the Laboratory of Physiological Hygiene, University of Minnesota, where he has been doing work on behavior in nutritional studies, visual performance and fatigue, and personality factors in hypertension. He is co-author of The Biology of Human Starvation *(U. of Minn. Press, 1950). Dr. Brožek's present article is an outgrowth of his interest in the history of science and the organization of research.*

VAVILOV, president of the Academy of Sciences of the USSR, recently published a volume of essays largely devoted to the history and the prospects of Soviet science.[1] An attempt will be made here to present the essence of Vavilov's thought and to provide information not easily accessible to American scientists, rather than to evaluate critically Soviet achievements in science or the goals and organizational aspects of the five-year plan. The potential usefulness of Vavilov's little volume is enhanced by the fact that it includes a survey of research problems proposed for investigation during the 1946–50 five-year plan of the Academy of Sciences, the mammoth research institution occupying a dominant place in Soviet planned research.

The Academy of Sciences was established at St. Petersburg in 1725 and became the cradle of Russian science. It is hard to exaggerate the contrast between the Old Russian culture, Byzantine in content, tradition, and inertness, and the spirit of Western science, the science of Galileo and Newton. The academy was to provide an intellectual "window to the West." At first the work was carried on by foreigners, such as Euler and Bernouilli, who became members of the academy, but in time seed took root in the native soil. Lomonosov—a genius of rare magnitude, chemist and physicist, linguist and poet—the mathematician Lobachevski, the chemist Mendelyeev, the physiologists Sechenov and Pavlov, have a secure place in science's hall of fame. Some of these workers were associated not with the academy but with other centers of scientific activity which sprang into existence in the course of the nineteenth century. Universities were created in Moscow, Kazan, Kharkov, and other cities. The Military Surgical Academy was founded in St. Petersburg.

Although individual Russian scientists had made first-class contributions, the total scientific output remained low. There was a lack of continuity of research and of proper facilities. Moreover, the work was not adequately supported. The Russian *globus intellectualis* showed many a "white spot," and the application of the results of scientific research was slow and ineffective. Russian mining and manufacturing industries depended almost completely on foreign production techniques and foreign machinery. Many consumer goods had to be imported. Even military equipment was obtained largely from abroad.

From 1917 on a remarkable quantitative growth of science took place in the USSR. Before 1917 the number of scientific workers in all fields was estimated at about 1,000. In 1946, the ranks of senior scientific personnel (doctors and professors) rose to about 10,000, with 23,000 "candidates of science." Today Soviet scientists attempt to cover all the major aspects of science. This uninterrupted scientific and technical front is regarded as a necessity because of the special social and economic conditions of the country and the international situation, which are considered unfavorable to scientific cooperation.

Development of Science in the Early Years of the Soviet Union

The development of science in the early years following the October Revolution of 1917 suffered from the ravages of the civil war and its consequences. At the same time, the efforts to raise the standard of living led to a strong emphasis on the place of science and technology in the reconstruction of the country's shattered economy and in strengthening military preparedness for defense. In 1918 Lenin cited a rational distribution of industry, the electrification of the transport system, industry, and agriculture, and the utilization of water and wind as sources of energy as the most important objectives of Soviet technology. Scientific and technological research and development were liberally supported, and organized along new lines. Some of the new, highly specialized research institutes reached gigantic proportions. The Central Aerohydrodynamic Institute, created in Moscow, grew out of the small prerevolutionary aerodynamic laboratory of Professors Zhukovski and Chaplygin, who had been concerned with abstract, highly mathematical problems of aerodynamics. The institute came to play an important part in the development of Soviet aviation. Other new organizations for research and development in Moscow were the Physics Institute and the All-Union Electrotechnical Institute; in Leningrad there were established the State Optical Institute, Roentgen Institute, Radium Institute, etc.

The old Academy of Sciences changed profoundly. Its laboratories were transformed into large-scale research centers (Kurnakov's Institute of Physicochemical Analysis, Pavlov's Physiological Institute, Steklov's Physicomathematical Institute). New universities and schools of university rank were established.

Soviet research workers made important contributions to the development of the country's resources. Thus systematic study of the so-called Kursk magnetic anomaly led to the discovery of large deposits of iron ore. Fersman's investigations in the Far North, on the Kola Peninsula, and the discovery of large apatite-nepheline deposits became a starting point for the development of a whole new industry. In time, the Soviet Union could rely on its own industry for the manufacture of electrical equipment, lamps, optical glass, and optical instruments, all of which had to be imported from abroad in the days of czarist Russia. There was a significant rise in the quality and quantity of production of airplanes, automobiles, and railroad engines.

Soviet lamps, tea, and even the lowly aspirin, represent important achievements and contributions of Soviet science and technology to the raising of the standard of living in the Soviet Union, even though the general level continued to lag far behind that of the West. In Soviet agriculture, increased productivity, introduction of new varieties of grains, and their acclimatization under unusual conditions (e.g., in the Far North), would have been unthinkable without the research of Soviet biologists and agriculturalists. In physiology, Pavlov continued his work on conditioned reflexes. Rozhdestvenski and Fridman were concerned with the structure of the atom and the theory of relativity. Joffe made advances in the physics of crystals. Marr worked in linguistic theory. The languages and the history of the multitude of nationalities constituting the Soviet Union became important topics of research.

Science in the First Three Five-Year Plans

A new phase in the development of Russian science was introduced by the establishment of the ambitious five-year plans, aimed at a more speedy realization of the goals of a socialist economy. The first five-year plan was promulgated in April 1929. Scientific research was also put on a planned basis. Vavilov's discussion of the basic problems of planning in science should be read by both the protagonists and the antagonists of the idea of planned research. Vavilov takes up as a challenge the thesis put forth by A. K. Tolstoy that "We cannot control the march of the sciences, only sow their seeds," a thesis interpreted as a skeptical attitude toward planning of scientific research. Vavilov is well aware of the fact that basic discoveries and fundamental theories—Darwin's theory of natural selection, Mendelyeev's concept of the periodicity of elements, Becquerel's discovery of radioactivity, Pavlov's theory of conditioned reflexes, Einstein's theory of relativity, Bohr's quantum theory—cannot be "planned." But, he maintains, much of scientific research is pedestrian in character, and the detailed exploration of partly known areas can be pursued according to a plan laid ahead of the actual work.

It appears that significant discoveries, opening new horizons, have not been lacking during the years of the five-year plans. Vavilov, himself a physicist, draws his examples largely from his field of specialization. He points to Skobeltsyn's research on cosmic rays and the disintegration of the atomic nucleus; Mandelshtam and Landsberg's discovery of combinatory dispersion of light, made simultaneously by Raman in Calcutta, and adding significantly to our knowledge concerning the nature of light and of matter, as well as yielding a method

which found practical use in the analysis of a number of organic substances, especially of distillate fractions of crude oil; Fok's method for calculating the structure of complex atoms; Cherenkov's discovery of the unusual optical properties of fluids bombarded by the radium's beta or gamma ray; etc. Important progress was made in many technological fields.

By this time the organizational pattern of Soviet research became well established, with four categories of research institutions ranked in decreasing order of importance:

1) The academies (Academy of Sciences of the USSR, eight academies of sciences in different republics, Lenin Academy of Agricultural Sciences, Academy of Medical Sciences, Academy of Pedagogical Sciences, Academy of Architecture). The structure and function of the Academy of Medical Sciences were described by Shimkin,[2] and its five-year plan has been presented in some detail in English.[3,4]

2) Institutes of the Ministries, such as the Ministry of Health and of Agriculture.

3) Universities and schools and research institutes of university rank.

4) Factory laboratories, agricultural research stations, and hospitals—devoted to applied and developmental rather than fundamental research.

The war served as a dramatic and merciless test of Soviet technological achievements, and of industrial and agricultural productive capacity. One should keep in mind that World War II was to a large degree a war of science and technology. The fact that more than 70 per cent of wounded soldiers were returned to duty was cited by Vavilov as an indication of the high level of medical science and service. Transplantation of the cornea, efficient transfusion of blood plasma stored in blood banks, treatment of frostbite, and manufacture of new drugs were noted among successes of Soviet medicine. Vavilov is proud of the construction and effectiveness of Soviet guns, tanks, airplanes, and ships. The advances in optics, chemistry, and geological research contributed much to the success of military operations.

The Fourth Five-Year Plan

The current five-year plan, scheduled for 1946–50, has for its objective the reconstruction of the parts of the Soviet Union damaged by the Nazi war machine, and an over-all intensification of industrial and agricultural production. Strengthening of the material basis of Soviet science is considered an important task. During the war there were losses in scientific personnel, the buildings of some scientific institutions were demolished, and their equipment and libraries severely damaged or carried away.

It may be of interest to enumerate some of the conditions considered by Vavilov as necessary for a healthy growth of Soviet science:

1) An increase in the number of well-trained scientists, capable of independent research and familiar with specialized research techniques.

2) More space for the research institutes.

3) Good equipment built in the Soviet Union, a plentiful supply of reagents, specialized libraries containing relevant international literature covering the field of specialization.

4) Efficient shops for mechanical work, carpentry, glass blowing, optical and electronic work.

5) Rapid publication of scientific results that are not of a confidential character.

6) Close contact of science with technology.

7) Effective organization of the research institutes.

8) Material security of the scientist, making it possible for him to concentrate his energy fully on scientific work.

9) Better training of technicians (assistant scientific personnel) and improvement of their material conditions.

10) No interference with research by organizational and administrative groups.

An important contribution to the reconstruction of the laboratories will be made by the special commissariat for construction of machines and equipment in which a section was established for making scientific equipment. Its task is to supplement, not to replace, work done in shops associated with existing research institutions. The provision of laboratory supplies is one of the tasks of the commissariat for chemical industry.

Science, in its turn, is to contribute more effectively than ever to the utilization of natural resources and the defense of the country. The fulfillment of this plan calls for what amounts to a mobilization of scientists. The harnessing of atomic energy has a high priority on the list of objectives of scientific research and development. Marked increase is expected in the production of pig iron, steel, coal, crude oil, electrical energy, wood, construction materials, and agricultural products. Other goals include intensification of geological prospecting surveys, especially in the eastern part of the USSR; increasing the efficiency of processes for the concentration of low-grade ores; introduction of new techniques in the production of elec-

trical energy (e.g., application of steam at high pressure and high temperatures); solving problems of efficient transmission of electrical energy over large distances; and production of apparatus for spectral analysis and of other optical equipment.

The Academy of Sciences and its Current Research Program

The Academy of Sciences, the oldest Russian scientific institution, has changed greatly since the establishment of the USSR. As early as 1918 Lenin visualized that the academy, which up to that time had little contact with the life of the country, should become a scientific consultant and collaborator of the government. The Academy of Sciences plays, quantitatively and qualitatively, an important role in present-day Soviet scientific endeavor. The perspectives for its work indicate the general trends.

In 1949 the academy published some forty periodicals. There were nine journals dealing with different aspects of chemistry, three with physics, and six with biology. The academy served as a publisher for some of the scientific societies (e.g., Mineralogical Society, Geographical Society, Society of Physiologists, Biochemists, and Pharmacologists). Separate proceedings are published by the divisions of literature and languages, technological sciences, chemical sciences, economics and law, biology, geography and geophysics, geology, history and philosophy, mathematics, and physics.

The academy is concerned primarily with broad fundamental problems, not with concrete, purely practical tasks. For example, it would deal with the problems of transmission of radio waves but not with the construction of equipment for broadcasting. The work of its Technological Division, with its multiplicity of problems, will not be considered. According to Vavilov's report, the five-year plan of the Academy of Sciences, presenting the material in a condensed form, takes up some 1,500 printed pages, of which only the main lines can be indicated here.

Close attention is being paid to the philosophy of science, considered as the cornerstone of the total structure of Soviet science. This is the single most important difference between the Eastern (Soviet) and Western scientific atmosphere. It is considered by Vavilov as an essential component of the five-year plan that the philosophy of dialectical and historical materialism should become more closely and concretely integrated with Soviet science. This philosophy should enter militantly the international area and demonstrate its power and vitality in competition with the inimical philosophical systems developed abroad.

Closely related to the problems of philosophy, particularly logic, is mathematics. Mathematics is applied as an essential tool in natural sciences, in technology, and in such social sciences as economics. Considerable effort is to be directed during the current five-year plan toward a more effective utilization of mathematics in other sciences. This includes provision of calculators utilizing the possibilities opened up by electronics and facilitating solution of complex mathematical problems, including differential equations. It is realized that a calculator, however complex and ingenious, does not replace the creativity of a mathematical genius. Mathematical theory, requiring a combination of daring fantasy with strict discipline of the mind, will continue to be cultivated intensively. The story of non-Euclidean geometry, of tensor calculus, of the theory of groups, demonstrates that highly abstract mathematical fields may attain, at a certain stage of scientific development, a decisive importance in scientific research and the solution of technological problems. The five-year plan provides for work on the theory of numbers, methods of mathematical logic, abstract algebra and topology, etc.

In these days of atomic energy, radar, and jet-propelled aircraft, physics in the five-year plan has no need to clamor for recognition. Physics has become the basis of other sciences as well as of important branches of technology. Its central present-day problem is the intimate structure of matter, its elementary particles, their characteristics and interrelationships. The atomic nucleus and cosmic rays represent the core of the most interesting theoretical problems, as well as important bases of future technology. At the same time, work is going on with reference to the physical properties of aggregates of matter (gases, fluids, solids).

In some sectors the lines between contemporary chemistry and physics have become very tenuous. The kinetics of chemical reactions under different conditions is such a borderline field. The projected chemical investigations are centered on the structure of molecules and the theory of chemical affinity elaborated in the framework of contemporary quantum mechanics. The chemistry of processes taking place under the influence of light is considered an important sector in the advancement of chemical theory. Electrochemistry, the study of absorptive processes, and colloidal chemistry are expected to provide new information vital for the industrial part of the five-year plan. Organic chemistry is closely bound with industry and agriculture, and it constitutes a bridge to biology and medicine.

In biology, the questions of protein structure,

virus activity, biosynthesis, and antibiotic agents, such as penicillin and streptomycin, have the highest priority. Problems of evolution and heredity, including their physicochemical aspects, and the relationship between the organism and its environment will be investigated intensively. Work will be continued on the physiology of higher nervous activity, following Pavlov's approach, and on the psychophysiology of sense organs.

In astronomy, the reconstruction of observatories demolished by the Germans—such as those at Pulkovo, Poltava, Simeiz on the Crimea, and elsewhere—will constitute the central task, which must be carried out in cooperation with optical and mechanical industry. The existing facilities will be used for the study of the stellar systems and the metagalaxies, the physics of the stellar atmosphere and of the sun, and celestial mechanics. The study of radio waves of cosmic origin is expected to provide important new data on the galactic system.

The geophysicists will continue to investigate seismic and magnetic phenomena in the Soviet Union and improve geophysical methods of prospecting, which will utilize electric, magnetic, radioactive, seismic, thermic, and other phenomena. They will collaborate in the extensive system of hydrometeorological service and weather prediction. The geologists will pay increased attention to the Pacific coast and the Pacific Ocean, and to the search for scarce ores, minerals, and crude oil. Geology, geography, and the chemistry of the soil are important sectors in the five-year plan.

The strong rise of patriotism during the second world war gave impetus to intensive historical research. Ambitious works were proposed on the history of the Soviet Union, its arts and sciences, and its prominent personalities. The academy is an outstanding center of linguistics; included in the plan is the compilation of a thesaurus of contemporary literary Russian and of a historical dictionary of the Russian language. Work is to be continued on grammars and dictionaries of non-Russian peoples of the Soviet Union, and attention is to be paid to Oriental studies. In the field of literary science, large historical works are planned dealing with the history of Russian literature and Russian lyrical poetry, as well as complete critical editions of Lermontov, Belinski, and Gorky. The economists will be concerned with the general problems of planned economy, the question of wages in the USSR, industrialization, increased production in agriculture, political and economic changes in the capitalist countries since the war, the role of government in the economy of the USA, etc. In the Law Institute of the Academy such topics will be under study as the rights to property and to work, and compensation for work; guilt under the Soviet criminal law; and several current problems of international law.

Vavilov stresses not only the relationships between science, technology, and the economy but also interrelationships between sciences. Emphasis is placed on the importance of research in areas which are on the borders of two or more sciences, and on integrative research. He points out the increasing importance of optical methods (such as the determination of spectral and luminescent characteristics) for the analysis of the chemical constitution of the substances. He deplores the excessive departmentalization in the investigations of the earth, and the lack of effective articulation between geophysics, geology, aerology, and the study of the fauna and flora. It is his belief that application of the new techniques of physics and chemistry will result in the near future in profound progress in our understanding of the life processes. The progress in biology will be reflected, in its turn, in medicine and agriculture and, possibly, will enrich physics and chemistry as well. Research on atomic energy is opening new horizons and new tasks for all natural sciences, from physics and chemistry to biology and medicine.

An attempt to arrive at a picture of Soviet science as a whole, necessary though it is for the understanding of particulars, is fraught with difficulties. The time of pansophists, equally versed in all aspects of knowledge, is irrevocably past. Ever-increasing scientific specialization narrows the range of one man's competence to ridiculously small dimensions. The present writer felt this limitation only too keenly and was aware also of Vavilov's own handicap.

In dealing with the general aspects of science, the danger of confusing presentation of facts and their evaluation is serious. The more controversial the topic, the greater the danger of looking at the subject through colored glasses, frequently without being aware of it. The severely limited information on the development and, particularly, on the present status of science and technology in the Soviet Union available to Western scientists and historians of science increases the danger of acceptance of a biased presentation. In English literature Ashby's *Scientist in Russia*[5] should be noted as a dispassionate and well-informed treatment of the subject.

Vavilov's essays, on which this article is largely based, were designed for local consumption, not for export. That increases in an important way

their documentary value. At the same time it must be realized that events and trends were seen by Vavilov from his particular point of view. He occupied a strategic position in Soviet science and wrote ex cathedra. Nevertheless, his 1946 view of science and its function, organization of research, the standards of scientific truth, the international character of scientific endeavor, etc., would have been shared in a large measure by Western scientists even though they would reject some of the philosophical premises as containing dangerous seeds of rust and rot. Since the time of his writing there has been a marked recrudescence of dogmatism and of Communist ideological warfare, coupled with extreme chauvinist attitudes. This was most marked in genetics and biology,[6] but is likely to permeate all sectors of science, including physics, and to widen further the gap between East and West.

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THE MOON

(THOUGHTS FROM AN OBSERVATORY)

The earth-polished,
The star-searching lens, with sight
Thrust out beyond the shallow windows
Of the insatiable brain,
Sees a dead satellite,
Uses a language of inequalities,
Of the parallactic equation, and the variation.
But into this territory of the moon
A poet, tuned to heavenly explorations,
Wandered first,
Leaving his own geography
Under the cerebral numbers
And the clear and austere symbols.
For, long ages before the Eye of Palomar
Had opened,
Giovanni Battista Riccioli, he
Of the Society of Jesus,
Composed the place-names of the moon
And wrote them down during the dark watches
Of the night, between the first still nocturne
And the last. (What desert death
The poet's phrase redeems!)
That shadow,
There between those two wan plains, he marked
"Sea of Tranquillity," and there inscribed
"The Valley of Endymion."
And what, for clearer sight, is now
An arid sink, he called
"The Lake of Dreams."

Gemma d'Auria

Some Observations on the Economics of Atomic Power

JOSEPH E. LOFTUS

Mr. Loftus is director of the Teaching Institute of Economics at the American University, Washington, D. C. During the past two years, the Institute has concentrated its resources on an extended examination of the economic aspects of atomic energy utilization. This paper is one of a series of studies developed in the course of the Institute's work.

ONE of the premises of a recent analysis of the problems of international control of atomic weapons is a fourfold proposition: the development of atomic power is at least twenty years distant, there is no clear showing that atomic power in most instances will be cheaper than conventional power, commercial deposits of uranium and thorium are relatively rare, and atomic power because of its comparatively high costs will have only limited economic effects.[1] If this premise is correct, then it follows that at least for a decade or two consideration of the economic issues of atomic power can be safely postponed—thus reducing the international control problem, for the time being, exclusively to political and military considerations. If, on the other hand, the premise is incorrect, then any limited-production type of control proposal[2] which ignores the incipient economic realities of the situation is fated to encounter considerable resistance and, quite possibly, rejection. Accordingly it is vital that answers as dispassionate and as correct as the obscurity of the future will permit be given to the questions: When will atomic power be developed? What will it cost? Is there enough uranium and thorium available to sustain a large-scale development? What will be the character and magnitude of the economic effects of atomic power? Since all these questions are large ones, only the first three can be considered here.

When Will Atomic Power Be Developed?

Before attempting to forecast when the generation of electric power from atomic reactors will be accomplished, it is essential first to review carefully the progress thus far and then to analyze and evaluate the factors that have influenced the rate of development.

The main highlights of the reactor development program, 1945–50, can be quickly summarized. In the first place, "since the end of the war, no really new or greatly improved versions of reactors have been built in this country."[3] Now, at long last, after ". . . years of discussion and argument as well as from new knowledge,"[4] three experimental reactors are in the embryonic stages of construction: an experimental fast breeder reactor, an experimental land-based prototype of a submarine propulsion reactor, and a materials testing reactor.[5] Of these three, the first "will perhaps be finished in 1951," and the others will probably follow close behind.[6] It must be noted that these reactors are but experimental reactors designed to test, empirically, hypothetical solutions of the technical problems involved. On the basis of knowledge obtained from the observation of the operation of these reactors, a new generation—or generations—of reactors will have to be designed which may or may not terminate the first big stage in the development process.

This in brief has been the record of the reactor development program in this country. It has become common practice—both within and without the AEC—to attribute the slow rate of development to the existence of many unique and difficult technical problems.[7] As was anticipated from the outset,[8] and reiterated repeatedly,[9] the ultimate speed and success of the development program will be dependent upon the solution of the inherent technical problems. In this preoccupation with the ultimate necessity of overcoming the technical difficulties, however, what has become obscured is the hard fact that the solution of the technical problem is itself influenced by many other factors.

In the first place, there is a matter of priorities. It needs no elaborate demonstration to indicate that at least until recently the reactor development

program was a minor element in the total atomic energy program. Because of the exigencies of a tense world situation, the preponderant part of the Commission's program was oriented toward weapons development and production. In terms of costs incurred, as late as fiscal 1949, actual expenditures on reactor development constituted slightly less than 9 per cent of the total Commission budget; the comparable estimated expenditure for fiscal 1950 is only slightly more than 9 per cent.[10] This, clearly, in terms of utilization of facilities and skilled manpower, is not the energetic sort of effort assumed by the post-Nagasaki optimistic forecasts of the advent of atomic power. Basic to the majority of those forecasts was the assumption of an "all-out" attack on the problem, comparable to the Manhattan District project.

Second, basic to the whole problem of reactor development is the question of risk bearing—and this has its personality, political, and financial aspects. As Dr. Hafstad has correctly pointed out, there are many risks involved in any new technical development. Sooner or later it becomes necessary "to find the courage to build a few reactors, to test what we think we know."[11] How much courage measured in dollars? Dr. Suits, director of research for General Electric, has estimated that "the Government would have to spend about 200 million dollars to build a number of successive experimental atomic power plants before the economic possibilities of atomic energy could be judged."[12] The construction of the first three reactors now authorized will cost approximately sixty million dollars,[13] and this is only the beginning of the experimental period. Nor is this a question of money only; it also involves critical decisions to invest "scarce" fissionable materials in experimental reactors rather than in the military stockpile.

This problem of risk bearing is a subtle thing. Without a clear mandate from the American public, from the Administration, or from Congress, the Commission has been placed in the peculiar and precarious position of simultaneously being not only the technical engineering department of the enterprise but also the formulator and assumer of the basic risk-taking policy decisions of that enterprise. It is only against this background that an otherwise curious statement of Dr. Bacher's makes profound sense:

> I think I can say that the reactor program has not gone ahead as fast as we had hoped it would. . . . But I believe today we stand on the threshold of a very great development in this field. I am certain that *if we—and by "we" I mean the people of the United States—are not timid in going ahead with this work* [italics mine], I believe that major successes will come to us, but timidity and playing things safe are not a background for atomic energy development.[14]

In addition to these two central influences (the problem of risk bearing and the question of priorities), at least a few other minor factors have impeded the solution of the basic technical problems. In the first place, certain significant delays were necessarily encountered in the transition of the whole atomic energy program from the Manhattan District to the Atomic Energy Commission. Second, even after the transition was completed, the AEC went through a series of painful internal reorganizations before the reactor development work was centralized into one unit with clear responsibilities and authority.[15] Finally, the new division itself went through a costly maturation period before it felt sufficiently sure of itself to take such bold steps as "freezing designs"[16] and getting on with the business of actually constructing and testing a few experimental reactors.

With this as a background it now becomes possible to make a first approximation to the question, when will atomic power become practical? Of the three experimental reactors now authorized, the AEC has indicated that the experimental fast breeder will be completed sometime during 1951; the other two sometime in late 1951 or early 1952. Since all three are by definition experimental reactors, it is necessary to assume that in all probability at least a year or two will have to elapse before ample and useful empirical "performance" observations can be collected and interpreted. Translated into a rough calendar schedule, it is not likely that the design of a new generation of reactors will begin much before mid-1953 or early 1954.

Assuming that past performance rates will not be changed, we can postulate a minimum period of two and a half years for the designing of new reactors, and an additional minimum of two and a half years for actual construction based on the new designs.[17] Accordingly, at present rates of development in this country, it hardly seems likely that a second generation of reactors could come into being much before 1958 or 1959.

If these reactors should prove to be technically sound and economically feasible, then a rapid development of the atomic power industry might ensue after the turn of the decade. If, however, the behavior of the second series of reactors reveals substantial technical imperfections and economic disadvantages, then the process would again have to be repeated—thus postponing the advent of commercial atomic power until at least 1967.

Thus far this first approximation has proceeded on the basic assumption that there will be no change in the historical pace of the reactor development program. On this assumption, the advent of atomic power will not appear much earlier than 1958 or 1959, and if only limited results are then attained it might well be delayed until at least 1967. The questions that now must be examined are: Can this pace be accelerated, and will it be accelerated?

With respect to the first question, at best only a few months could be shaved off the present timetable for the completion of the first reactors. This could be accomplished by the imposition of more urgent priorities on the actual construction work.

Likewise, only a year or two at the most could be shaved off the present estimated schedule for the construction of the second generation of reactors. To accomplish this, the AEC would have to expedite the construction of the first generation of reactors, shorten the period of empirical observation, initiate work on new designs even before the completion of the period of empirical observation, and then, if there were felt to be sufficient urgency, to initiate—after the fashion of the Manhattan District project—actual construction of parts of the second generation of reactors even before the design work has been completed. If all these things were done, it is just remotely possible that the second generation would come into being around late 1956 or 1957. By a curious coincidence, this is approximately the same date that must have been envisaged by Mr. Lilienthal in 1947 when he stated, "A common estimate of the time schedule for the development is from eight to ten years to overcome the technical difficulties and have a useful practical demonstration plant in operation."[18]

With respect to a third generation of reactors—if such becomes necessary—considerable latitude exists for shortening the now indicated schedule. Essentially what would be required is the initiation of research, design, and limited testing of models for the third generation of reactors even before construction of the second generation of reactors has begun. It is both interesting and promising to observe that in its 1951 budget the Commission has requested funds to construct a small-scale homogeneous reactor[19]—this is one of the brightest prospects for the future of safe, economical atomic power. Although it is utterly impossible to assign any figure to the number of years that could be saved by this type of action, it would not seem too arbitrary to set the figure at five years. Thus, under the most favorable circumstances, a third generation of reactors could come into being by 1962.

Whether there will in fact occur in this country this sort of speedup will depend largely on how intensely the nation wants atomic power. Although it is never easy to assess the wishes of a nation, nevertheless several recent developments are not without significance.

In the first place, shortly after the climax of the recent heated discussion of the hydrogen bomb question, it was quietly announced that the development of atomic power for the propulsion of submarines would be given "every possible priority short of stopping actual work on the H-bomb project."[20] This would not be the first time in history that a military requirement served as the *deux ex machina* for the acceleration of a peacetime development. By a curious irony, it so happens that the problems and prospects for military and peacetime utilization of atomic power are intimately intertwined.

In the second place, there is some little recognition that one of the implications of the Beria bomb is that if this country is to maintain "atomic supremacy," it must not permit any other nation to surpass it in a well-evidenced demonstration of its primary concern in the peacetime applications of atomic energy. If any other country—especially the Soviet Union—should more quickly develop a full-scale reactor producing electric power for all the world to see, the rewards to that country and the losses to this, measured in terms of international prestige (particularly in the politically marginal countries of Europe and Asia), might be incalculably great.[21]

Third, there is a growing recognition within and without the Atomic Energy Commission that we must get along rapidly with the development of atomic power. For example, in the preface to the presentation of the reactor development budget to the House Subcommittee on Appropriations this year, the Commission has departed from its several-year-old disposition to emphasize the peaceful uses of isotopes and minimize the prospects of atomic power by stating quite bluntly. "From the long term point of view, the development of reactors is *the main hope and almost the only hope* [italics mine] of any appreciable use of atomic energy."[22] It is also not without significance that Mr. Lilienthal, speaking at Town Hall shortly after his resignation from the Commission, asserted most emphatically that

> We must recognize as soon as possible the importance of the development of atomic energy as a new and vital

> branch of our industrial technology, comparable with the technology based on chemical energy from coal and the internal combustion engine . . . that we must get away from dependence on coal and should begin to throw the energies of the country into the development of nuclear energy on a large scale. . . .[23]

Whether the operation of these and other influences will result in the acceleration of the national effort to harness atomic power is difficult to determine. It is even more difficult to ascertain with any precision what the numerical magnitude of the acceleration will in fact be. The best that one can say is that with much luck, and even more perspiration, there is a very remote possibility that there might occur in this country an economically and technically successful demonstration of atomic power as early as 1957; conversely, with little luck and limited effort, such a demonstration might not occur until 1967. A date somewhere between these two extremes is more probable.

In a real sense, however, the assignment of a precise date is much less important than the recognition of what appears to this observer to be the central fact: namely, that how soon this country—or any other country—achieves atomic power is not simply a question of the existence of technical difficulties, but it is just as importantly a function of how much vigor and effort are directed at overcoming those technical difficulties.

One final word of caution. I make no claim that the dates mentioned above are infallibly accurate. Essentially, what is being urged is that policy makers in possession of more abundant facts apply this type of detailed analysis as an alternative to the blind *ipse dixitism* that has characterized so much of the well-intentioned thought on the international control problem.[24]

Atomic Power Costs

It must be emphasized at the very outset that nothing is known with numerical precision about the magnitude of atomic power costs. This is so because of the very simple fact that no such power plant is in existence to provide operating experience.

Thus, in the absence of precise numerical cost data, but in the face of the hard fact that for policy-making purposes there must be judgments on these matters, four types of estimates have come into existence and use. In the first place, there are quasi-quantitative studies—of which the so-called California Report[25] is the outstanding example—prepared by people with considerable background in nuclear engineering and industrial chemistry, but with no access whatsoever to the experiential data of the various Atomic Energy Commission installations. Such studies are interesting and even of some limited use in providing a rough idea of the potential economies of large-scale, highly developed atomic power plants at a far future date, but they must be contemplated with a cautious, skeptical eye. Even though the estimates are expressed in decimal place numbers, the important fact to bear in mind is that the figures were composed in an armchair and not in a power plant.

The second type of estimate is in a very real sense not an estimate at all—the forthcoming significant study of the Cowles Commission is the outstanding example.[26] The problems to which Messrs. Schurr and Marschak addressed themselves were: (1) how does one go about analyzing rigorously and systematically what the *economic effects* of atomic power may be, and, (2) given at least a first approximation analytical technique, what might some of the economic effects in fact be? In studying the second question, in view of the fact that no actual atomic power costs exist, they were confronted with the hard choice of either "guestimating" what such costs might be or establishing a *range* within which atomic cost might actually fall. They wisely chose the second alternative. In making this choice, necessarily, if they were to avoid getting bogged down in endless permutations or combinations, they elected to limit themselves to just three points on a range: a lowest, an intermediate, and an upper cost limit. In assigning numerical cost figures to each of these range points, they selected numbers which seemed most reasonable in view of the limited data available on the subject.

It could well be that atomic power costs will never become even nearly identical with any of the three points on their range. This is essentially irrelevant so far as the validity and eventual usefulness of the Cowles Commission Study is concerned. To repeat, its orientation is not toward "second-guessing" what future atomic costs may be, but rather toward the most difficult question, how does one envision—with discipline and rigor—what the *economic effects* of such costs might be once the nuclear engineers have supplied them?

The third type of study is the so-called Thomas Report.[27] In 1946, at the request of Mr. Bernard Baruch, there was prepared, under the general direction of Dr. Charles A. Thomas, of the Monsanto Chemical Company, a study of the probable costs of nuclear power based on the best information then available. Since at that time—even as

now—no nuclear plant was in operation, the Thomas group relied heavily on the performance characteristics of the Hanford plant, despite the fact that Hanford was not designed to produce power, and that even within the more limited framework of simply being a plutonium factory, it had many of the imperfections of a hasty wartime improvisation in a brand-new industry.

With this as a starting point, however, the Thomas group developed conceptually a modified Hanford plant that would operate at sufficiently higher temperatures to supply power and yet would not attempt to produce plutonium surpluses for export to either the military stockpile or primarily plutonium-consuming power reactors. Thus, even on a purely conceptual level, this Thomas power pile was a crude modification of an already crude plutonium-producing installation.

Nevertheless, despite the essential technological crudity of the concept, the Thomas group estimated, on the basis of informed adjustments of the actual operating information on Hanford, that such a rudimentary power pile could generate electric power at costs not excessively more than those currently obtained in modern coal power plants. Specifically, it was estimated that, "on the assumption that the [atomic] plant would operate at 100% of capacity . . . the operating cost of the plant would be approximately 0.8¢ per kilowatt hour . . ." whereas "the operating costs of a [coal] power plant, would be approximately 0.65¢ per kilowatt hour, again on the assumption that the plant would operate at 100% of capacity . . ." and that "*equality of operating costs between coal power plants and nuclear power plants would be reached if the coal cost $10 per ton*" [italics mine].[28]

The most significant point is not that the Thomas power plant would have higher costs than a modern coal station but rather that right from technological infancy, without the decreased costs that come from continued research and development and consequent technological improvement, atomic power costs, for a literally custom-tailored plant, would not greatly exceed coal station costs. As the report itself suggests, "It seems reasonable to expect that the future development of nuclear power will result in the standardization of design and construction, and a material reduction in investment and operating cost."[29]

The fourth type of study—of which the Isard-Lansing study is a conspicuous example[30]—concerns itself not with the question of what the numerical magnitude of atomic costs will be but rather, given the data of the Thomas and similar groups, what can be said now (*a*) with respect to the internal composition of total atomic power costs (e.g., the relationship of fixed and variable costs, economies of scale, etc.), and (*b*) what are the economic implications of these probable internal characteristics of atomic power costs.

In order to accomplish this objective, Isard necessarily had to construct an elaborate table exhibiting on a dollar basis the many individual items (e.g., interest, depreciation, fuel, maintenance, etc.) which are constituent parts of total costs. Furthermore, it was necessary for him to manipulate the original Thomas data to reflect more realistic plant factors than the 100 per cent levels postulated by the Thomas group.

When all these revisions are made, the Thomas Report estimate of total atomic power costs rises to 0.94¢ per KWH at 80 per cent of capacity and 1.45¢ per KWH at 50 per cent capacity operations.[31] Although it is a highly debatable question whether these manipulations were formulated in a manner consistent with the implications of the Thomas Report,[32] for Isard's study this is in a very real sense irrelevant. The orientation of his study was *not* toward developing a numerical figure for the total cost of atomic power but rather toward discussing "the implications of what can now be perceived about the probable characteristics of the costs of atomic power plants and to bring out certain economic relations that will be of importance when, through accumulated experience, nuclear engineers are in a position to make fairly exact cost estimates." With this clearly in mind, Isard prudently prefaced and concluded his study with the caution "*Little if any significance, therefore, should be attached to the numerical estimates of costs of atomic power used in this paper*" [italics mine].[33]

Accordingly, despite the fact that the Isard study does contain a numerical figure for total atomic power costs, this figure, paradoxically, is not at all an estimate of what total atomic power costs will be. On the contrary, it is, at best, only an analysis of some internal economic relationships between the major components of total costs. So long as the study is thus interpreted, it is a provocative document. So long, however, as it is misinterpreted to be an estimate of the magnitude of total atomic power costs,[34] it creates a pessimism about atomic power costs that is not sustained on the limited evidence available.

In summary, then, on the question of atomic power costs there are available no real costs, in the sense of costs based upon actual operating ex-

perience. Instead there are four types of estimates. The type represented by the California Report is interesting but of little reliability for policy formulation work in the short and middle run. The types exemplified by the Cowles and the Isard-Lansing studies are significant contributions—though in different directions—to the economics of atomic power, but do not provide a policy-making usable estimate of total atomic power costs. In neither case was this the orientation or effort of their analyses.

This, then, leaves students of atomic development and control policy only with the generalized qualitative conclusions of the Thomas Report. For what it is worth, it seems clearly to suggest that even during the first generation of full-fledged reactors atomic power costs will not greatly exceed coal costs, and that as the technology develops and as design and construction are standardized atomic power may well become an economically attractive new energy source. This general conclusion is substantiated by a similar study conducted independently by J. R. Menke at Oak Ridge in 1946.[35]

Granted that the Thomas Report may be technically malformulated or based on overoptimistic or unrealistic assumptions, nevertheless, until something more authoritative appears, it constitutes the best evidence available. It is reassuring to observe that the AEC in submitting its 1951 budget has requested an unspecified sum to undertake a "study of the economics of producing power from reactors *taking into account advancements in technology*" [italics mine].[36] It is to be hoped that this study will be conducted with speed and perspicacity, and that it will be made publicly available at the earliest possible moment. Such a study is of crucial importance at this time for intelligent consideration of atomic control policy.

Raw Materials Adequacy

Without making a positive judgment one way or another, the Atomic Energy Commission quite recently has correctly emphasized "for either civilian or military uses, the extent to which atomic power will prove practical will depend mainly on the adequacy of the supply of raw materials, both as to costs and as to the amount available".[37] Although this latest statement expresses no judgment (other than to highlight the obvious that the successful development of "breeders" will multiply, in theory, existing supplies—however large or small they may be—by a factor of "more than a hundred"), earlier statements of high AEC officials provide some grounds for optimism. For example, Mr. Lilienthal has stated categorically:

> Statements have been made and widely publicized, that there is only enough uranium ore to last a relatively brief period, and so the prospects of benefit from atomic power must go glimmering. This is simply not so. . . . As explicitly as national security permits, the Atomic Energy Commission wishes to state that it finds no basis in fact for these statements about extremely limited uranium ore supplies. . . . Instead the contrary is true. . . .[38]

Despite the authoritative tone of this assertion, the fact remains that a persistent skepticism prevails on the adequacy of uranium and thorium reserves. This skepticism springs from several causes: (*a*) Instinctively in matters of this high concern one tends to be skeptical of an obiter dictum unbuttressed by hard facts and numerical data; (*b*) what data are available, generally, take the form of quantitative estimates of prewar production rates and prewar knowledge of reserves, and thus are discouragingly low; and (*c*) since it is estimated that only a small percentage of the world's supplies are concentrated in a few rich deposits and all the rest is spread thin in extremely low concentrations, the fear has developed that the costs of working such low concentrations will negate the possibility of ever developing a large-scale atomic industry.

In approaching a reasonable judgment of the adequacy of uranium and thorium reserves, it is useful to recall certain salient features in the development of the petroleum industry. Prior to 1870, petroleum had only limited medicinal and certain rather esoteric religious uses. As a consequence, it was never a high-value product, and consequently there were severe economic limitations on exploration and exploitation activities. Although the employment of petroleum fuels as an illuminant around 1870 widened somewhat the economics of exploration and exploitation, these were still impeded because other competing illuminants (e.g., common candles) set an upper limit to which petroleum prices could rise.

The turn of the century, with the development of the gasoline engine, and especially the Churchillian example in 1913 of substituting, wholesale, gas-fired and oil-fed engines for other means of propelling military vehicles, dramatically raised cost-price relationships in the petroleum industry to a new, high plateau. The situation was further accentuated by the fact that the gas engine demand for petroleum was a specific, monopoly-form demand in the sense that no other fuel could serve the required purpose.

The results of this radical change in the demand structure were at least threefold. In the first place, because of the high value of petroleum in the Gas Age it became commercially feasible to engage in very expensive and intensive exploratory operations. For example, the seismographic technique of exploration, now popularly employed, could never even have been dreamed of during the medicinal period of the petroleum industry. Second, as a result of a change in the economics of oil exploration, vast additions were made annually to the known reserves of oil throughout the world. And, finally, the changing economics of the industry made feasible drilling techniques and depths that forty years earlier would have seemed opium-fed optimism. For example, whereas now oil wells three miles deep are not at all uncommon, two to three hundred feet was the maximum tolerable depth in terms of the cost-price relationships of the period when medicine was the primary end-product use of petroleum.[39]

By analogy then, it is not unreasonable to anticipate that the same sort of economic revolution that occurred in the petroleum industry will take place in the uranium and thorium industries. From prewar use of very low value product (e.g., thorium's main use was in making gas mantles) these two resources now have a high value product (bombs) and will in future years have an even higher value product (bombs and power). The crucial question to investigate is: In terms of the upper limits that might be set for atomic power costs by the levels prevailing in competing energy sources, how high could the costs of mining, refining, and purifying uranium and thorium rise without raising the total cost of atomic power to a plateau that would severely limit or preclude its economic utilization on a large scale? In the forthcoming Cowles Commission Study of the economic aspects of atomic energy, a most useful approach to this problem has been developed.[40]

Summarily stated, their demonstration starts from the twin assumptions (*a*) that the entire weight of uranium or thorium could be made into nuclear fuel, and (*b*) that one pound of either metal could be converted into approximately two and a half million KWH of electricity. Given this relationship and the wartime price of uranium at approximately $20, then the cost of uranium expressed as a cost per KWH of electricity becomes only 0.008 mills per KWH! If, because of a necessity of going to very low-grade uranium, the cost of uranium should skyrocket to $2,000 per pound, this cost expressed in terms of costs per kilowatt of electricity would still be only 0.8 mills. "At $2000 per pound, uranium would be no more costly in terms of its energy content than coal at about $1.50 per ton. We can evaluate these figures better when we note that electric utilities paid on the average close to $5 per ton for their coal in 1946."[41]

This tremendous leeway that uranium and thorium costs possess in terms of energy content is further magnified when one considers the fact that significant portions of the world's supply of supposedly "low-grade" deposits of uranium occur as traces in oil shales, phosphate beds, and some gold ores.[42] Concrete steps have already been taken by the governments of this country, the United Kingdom, and the Union of South Africa to exploit on a large scale the occurrence of uranium in the gold-bearing Witwatersand.[43] Although no disclosures have been made with respect to how the total costs of mining the gold-uranium ores will be allocated as between gold and the by-product uranium, it is obvious that any—however small—allocation of total cost to the gold production account will decrease the cost of uranium.

The same sort of potential exists with respect to uranium-bearing oil shales. Conceivably, such a joint venture (i.e., oil and uranium) would be especially attractive to this country for two reasons: it would decrease our growing dependence on overseas supplies for both energy resources, and the joint development would probably decrease the final cost of domestically producing each resource.

With these concepts for a background one can evaluate the commonly accepted minimum and maximum estimates[44] of world supplies of uranium and thorium with a most optimistic eye. Even if breeding multiplies the usefulness of uranium and thorium by a factor of 25 rather than the theoretically obtainable factor of 140, atomic power can take its place alongside coal and hydro as a major supplier of world energy requirements for many centuries to come.

Rather than worry about the adequacy of uranium and thorium supplies, a much more significant avenue of worried inquiry that scholars and policy formulators would do better to travel is the question of what manner of international tensions and political alignments will be created by the indicated maldistribution of uranium and thorium throughout the world. From all present appearances these basic materials promise (or threaten) to author a case study in the strategy of raw ma-

terials that will be unique for its intensity and complications.

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SCIENCE ON THE MARCH

EROS, AN IMPORTANT MINOR PLANET

EROS deserves a special place in the hierarchy of the minor planets, and now that place has been won beyond dispute. In 1898, when Eros was discovered, there were already more than four hundred minor planets known. The question had been raised by astronomers as to what value there could be in continuing the discovery and observation of these little bodies. Photography had been in use about ten years, and the rate of discovery was increasing; also, they required a tremendous amount of computation to keep their predictions and identifications reliable. Why not just forget about the whole pesky business?

But Eros changed all that. It was soon recognized that Eros approaches so close to the earth at certain times that it could be used to derive the solar parallax, or the basic yardstick of the solar system, with greater accuracy than ever before. The orbit of Eros is not so eccentric but that it can also be observed in other parts away from the earth, and therefore a very accurate mathematical representation of its motion can be derived. To this day no other planet has yet been found which is more satisfactory for this purpose.

One close approach of Eros occurred in 1930–31. An international program was organized, and many hundreds of observations of Eros were obtained from all parts of the world. These were collected and reduced to a homogeneous basis by Sir Harold Spencer Jones, the Astronomer Royal of England, so that the distance to Eros could be determined by the trigonometrical method—which means that a triangle is formed by Eros and two observers in different parts of the world, as far from each other as possible. They each observe Eros at a different angle, and from these differences it is possible to compute the distance to Eros. Sir Harold's investigation was completed in 1941, and it gave such consistent results that it was believed that the solar parallax was known very accurately.

An even more elaborate investigation of Eros has recently been completed by Dr. Eugene Rabe at the Cincinnati Observatory, using the dynamical method. This method depends upon the motion of Eros in accordance with the law of gravitation, and inquires at what distance away Eros must be in order that it can move in accordance with the observations made over many years, and yet at all times be controlled by all the gravitational forces which act upon it from the sun and the major planets. This method determines the solar yardstick indirectly, but it also provides a check on the accuracy with which the masses of the planets are known. If an inaccurate value is used for the mass in computing the forces, it will not be possible to compute a motion which agrees with the observations. On the other hand, the accuracy with which the observed and computed positions agree, combined with the amount of influence each mass has on the computed position, enables one to determine statistically how reliable the value of the mass is.

Extraordinarily reliable results from this dynamical method can be obtained only after a large number of observations has been reduced to a uniform system, and all the computed forces and motion are carried out very accurately over a long period of time. Dr. Rabe's investigation covers the years from 1926 to 1945. The observations were collected into 37 "normal places," of which 21 could be taken during 1930–31 from the work of Sir Harold Spencer Jones' investigation of the trigonometrical method. The computation of the "perturbations," the influences of the planets, had been completed by Dr. Rabe's former associate, Dr. Gustav Stracke, an astronomer at the Astronomische Rechen-Institut in Berlin.

Dr. Rabe's solution by the dynamical method contains 74 equations and 16 unknown quantities. Six of the unknowns are corrections to the orbit of Eros. These were very small, because the orbit was fairly well known beforehand. Six other unknowns refer to the corrections to the earth's orbit. These proved not to be important because they can be determined even better from data other than observations of Eros. It is necessary to include them in the equations, however, so that their effect may be allowed to operate. The remaining 4 unknowns were the corrections to the masses of Mercury, Venus, Mars, and the earth and moon. It was here that the surprisingly accurate results were obtained.

It was to be expected that the mass of the earth and moon could be determined; it is this very property of a close approach which makes Eros valuable for this problem. But it was not to be expected that Mercury and Venus exerted such an important influence as to betray their weights so definitely. Both

Mercury and Venus are without known satellites, so that the usual method of determining the weight of a planet cannot be applied to them. Up to now it has been necessary to rely upon the long-term effects of Venus upon Mars in order to find the mass of Venus. Here we have one of the curious facets of astronomy: we photograph Mars to see how much Venus weighs. Because the motion of Mars has not been worked out with all possible accuracy, the resulting mass of Venus was somewhat uncertain. For Mercury it was even more uncertain, the uncertainty amounting to one fourth of the total value. Now Dr. Rabe has obtained the following results:

1/mass of Mercury	= 6,120,000	± 43,000
1/mass of Venus	= 408,645	± 208
1/mass of Mars	= 3,110,000	± 7,700
1/mass of Earth + Moon	= 328,452	± 43

It will be seen that the uncertainties are greatly reduced and the percentage errors are very small. From the mass of the earth + moon system it is possible to derive

the Solar Parallax = 8″.79835 ± 0″.00039.

This corresponds to a mean distance of 92,915,000 miles to the sun.

The normal places were represented with an accuracy of ± 0″.24 for the probable error, or about one unit in the sixth decimal place. Finally, Dr. Rabe examined the residuals of 1930–31 and determined the "Lunar Equation" and the relative weight of the moon. He obtained L = 6″.4356 ± 0″.0028 and 1/mass of Moon = 81.375 ± 0.026. This agrees very well with the value L = 6″.4378 ± 0.0018 derived by Sir Harold from the trigonometrical method.

All these results seem to be very good in themselves, but they still leave the astronomers with a curious enigma. Earlier solutions that have been made for the solar parallax by the dynamical method give almost exactly the same value as Rabe's, even though their uncertainties were greater and they depended on different observations. But the same 1930–31 observations which predominate in Rabe's solution give

the Solar Parallax = 8″.790 ± 0″.001

by the trigonometrical method. The earlier trigonometrical solution from observations in 1901 gave a value which was larger than the dynamical value and differed by about the same amount. Since the results by the two methods are discordant by amounts which are much larger than their assigned probable errors, it must mean that one of them is affected by some kind of a systematic error that is not recognized.

So far no one has been able to locate the source of such a discordance. Thus, in spite of the appearance of these two monumental papers, there is still a challenging problem remaining, and it is important because it involves one of the fundamental constants of astronomy.

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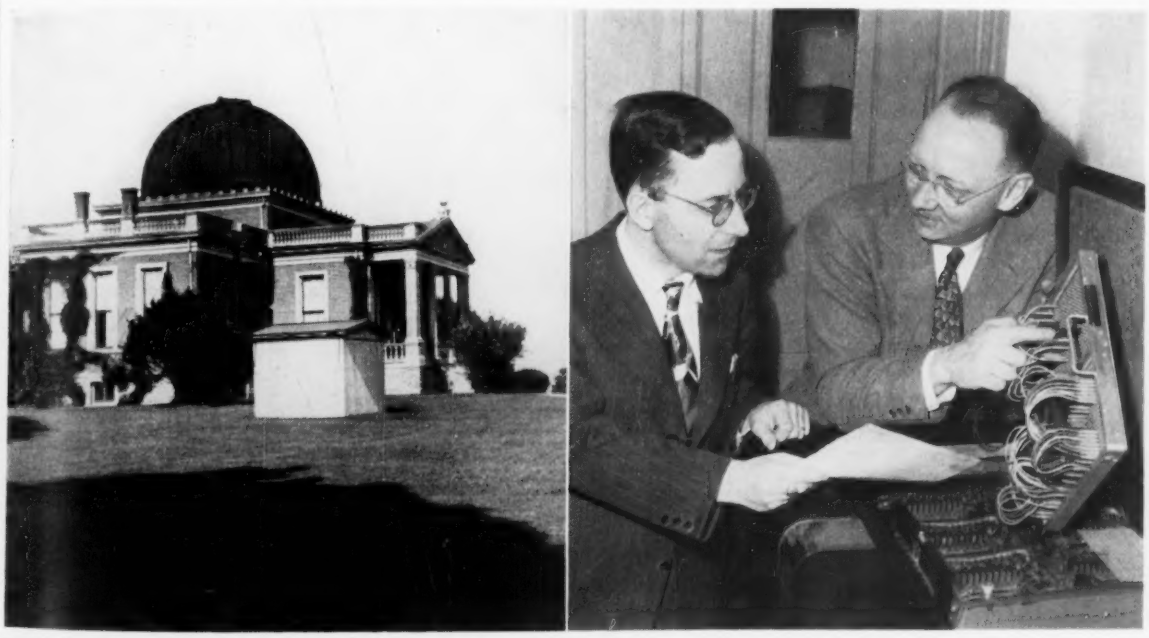

University of Cincinnati Observatory, center of asteroid research. Right: Drs. Eugene Rabe and Paul Herget at punch-card calculating machine, illustrative of the diversity of the work that has gone into the Eros program.

BOOK REVIEWS

ONE HUNDRED YEARS OF INDIAN HISTORY

The Indians of the Southwest. Edward Everett Dale. xvi + 283 pp. Illus. $4.00. University of Oklahoma Press. Norman.

NUMBER twenty-eight in the "Civilization of the American Indian Series," *The Indians of the Southwest* briefly recounts a hundred years of history of the vast territory acquired by the United States from Mexico in 1848, an area including California, Nevada, Utah, Arizona, and New Mexico. The account falls somewhat naturally into four chronological periods. That from the acquisition in 1848 till after the Civil War in 1868 demanded continued efforts toward the subjection of the various Indian populations. Between 1869 and 1900 some stabilization in the relations of Indians and whites was achieved. From 1900 to 1933 the Indian Bureau struggled with policy, administrators, and, of course, Indians, with somewhat doubtful results. The period from 1933 to 1947 was the longest during which a single commissioner held office, one which was revolutionary in many respects. Since 1947 the Indian Bureau has been proceeding on the momentum of the previous regime.

In this book the reader will readily find the important dates, and brief accounts of battles, compromises, treaties, policies, and laws. The last third of the book formulates the problems still faced by the Federal government; there are chapters that look to the future —The Agent and His Wards, Education and Schools, Health and Medical Services, A New Regime and Some Current Problems, Southwestern Indians and the Government in 1947.

One gets an impression of Indian problems as determined by private manuscripts, Federal and state documents, reports of commissioners and Indian agents, and investigating committees, miscellaneous books and articles. It is a picture painted by documents which could present only one, the white man's, side of the story, most of them written to defend action and expenditure of funds. There is little to indicate that the Indian might have a point of view.

As a member of the Meriam Committee of the Institute for Government Research, the author visited "every important reservation in the Southwest" (p. ix). The report published by this committee in 1928 brought about many reforms in Indian administration, especially in education and health. It had a great influence on the American public and paved the way for some of the changes that have been put into effect since 1933.

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MEDICAL CLASSIC

An Introduction to the Study of Experimental Medicine. Claude Bernard. (Translated by Henry Copley Greene.) xix + 226 pp. $3.00. Henry Schuman. New York.

CLAUDE BERNARD (1813–78) now occupies a rank in physiology equal to that of the renowned Pasteur (his contemporary and friend) in bacteriology. "He is not merely a physiologist, he is physiology." Nevertheless, although both French scientists made such incalculable contributions to human welfare that they became immortalized in the annals of medical history, Bernard has never been romanticized even for the "omniscient" schoolboy.

Like Pasteur, Bernard labored under extremely adverse circumstances: he had to work with crude, handmade instuments in a small, dark room, while guarding against police unpopularity at animal experiments. A prophet not without honor in his own country, he became a full professor, an Academy member, a winner of three Academy grand prizes, a teacher of distinguished research pupils, and a Senator under Napoleon III. It is not generally known that Bernard was buried with all the pomp of a public funeral at the expense of the state—a tribute previously paid to none but princes, statesmen, and generals.

Bernard was the founder of experimental medicine, a science wherein disease is produced artificially by means of chemical and physical manipulations, thereby enabling the biologist and the physiologist to study the problems of life, as well as the function of the organs in their normal and abnormal states. Eighteen octavo volumes contain Bernard's discoveries—puncture diabetes, pancreatic digestion, the vasomotor nerves, the glycogenic function of the liver, and the isolation of glycogen. Here he dealt also with experimental physiology in application to medicine, the effect of poisons, the physiology and pathology of the nervous system and of the body fluids, experimental pathology, anesthetics and asphyxia, diabetes and animal glycogenesis, and operative physiology.

Hence, it is fitting that at this time, when myriad laboratories are carrying on voluminous medical investigations, the publishers should issue still another readable reprint of Claude Bernard's *An Introduction to the Study of Experimental Medicine*, which was published first in 1865 and remains still an admirable philosophic guide to research. In fact, the classic is among the "100 Great Books" publicized by St. John's College.

This 1949 reprint contains an informative biographical obituary written by Paul Bert, one of Bernard's eminent students, and a preface (carried over from the 1927 re-

print) by L. J. Henderson, professor of biological chemistry at Harvard University, who correlates Bernard with physiology, the art of research, and medical science. The translator has rendered the original message smoothly, and the typographers have planned an interesting format.

The title of this classic should not mislead readers into expecting a technical treatise on physiology. Rather, the book is an essay on method; one "feels" Bernard on every page as he describes the successive steps of each of his principal discoveries. Most important of all is the fact that an interested reader, unable to learn the mystery from any other source, will find here how one man of genius honestly and successfully analyzes himself while at work on intricate problems. It is possible to see the parade of a scientist's purposes, feelings, curiosity, duty, contentment, and humaneness, and "the first condition for the progress of science is to bring them into play." A recommendation of Bernard's world-famous classic is in keeping with the trend of modern educators who teach that the importance of science in everyday living should be stressed as one of the major goals.

JULIAN M. SCHERR

The Medical Library of Bellevue Hospital
New York City

DEFINITIONS

Mathematics Dictionary. (Rev. ed.) Glenn James and Robert C. James, Eds. v + 432 pp. $7.50. Van Nostrand. New York.

THE scope of the first edition of this dictionary coincided with the standard course in mathematics, through the calculus, given students in our engineering schools. The present edition includes also basic terms in the theory of functions, theory of groups, analytical mechanics, statistics, etc. At the end of the volume one finds tables of logarithms, and others; also a table of integrals and a list of mathematical symbols.

Anyone looking up a term in this dictionary will find an explanation which is as clear and as reliable as one given in the average textbook that includes the term sought, and will locate this information with a great saving of both time and effort. And this is all the user of the book can ask for or is entitled to.

No useful purpose is served by giving essentially the same explanation under two different headings, such as "False position" (p. 144) and "Regula Falsi" (p. 302), when a cross reference would do as well. The term "local value," being listed under "value" (p. 373), there is very little need for a separate entry "local" (p. 218). The compilers could also have saved a good deal of space by omitting terms about which they have no more to say than any English dictionary, terms like "debt," "million," "ton," etc.

The space saved could be used to avoid such inconsistencies as including Menelaus' theorem and leaving out Ceva's, or stating the problem of Apollonius for three circles without mentioning the corresponding problem of four spheres.

The usefulness of the dictionary is attested by the fact that within a few years the work is coming out in a new and enlarged edition. The editors may be congratulated for having had the courage and the foresight to fill an existing need. May their example stimulate our excellent and active mathematical organizations to revive the idea of publishing under their auspices an encyclopedic dictionary of mathematics, an idea that has been floating around for many years and that was very dear to the late E. R. Hedrick, among others.

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KARTABO

Edge of the Jungle. (Rev. ed.) William Beebe. viii + 248 pp. Illus. $3.00. Duell, Sloan and Pearce. New York.

IN THOSE rare years when William Beebe does not have time to write another book, his publishers print a new edition of an old one. The present edition of the work first published in 1921 is as vivid as when it first appeared. Kartabo has not changed, and neither has the life of its jungle residents.

The Spaniards came three centuries ago, then the Dutch. The former left the keel of a ship, the latter left part of a stone wall. The English followed. When Beebe came in 1921 Kartabo had been largely abandoned, and second-growth forest covered the old settlement sites. Beebe said: "The ebb tide has left two successful crops, crime and science." Moving into an abandoned bungalow, shaded by bamboo trees brought three centuries ago and planted by the Dutch, Beebe established a household and commenced looking at things.

There was plenty to look at, and much of it he described. Plant life, small animals, birds, vampire bats, snakes, frogs, and bumble bees come to life in Beebe's fine prose. Army ants have their home town nearby, and, despite the fact that volumes have been written on these creatures, their guests, and parasites, Beebe's observations and description are classics. Again, there is the fungus-growing ant *Atta* described and named by Fabricius and written about and discussed for 146 years; it remained for Beebe to draw a delightful detailed account of its "jungle labor union."

His hammock, the ubiquitous sleeping apparatus of the tropics; his carriage in the Georgetown Botanical Gardens; his canoe; even the deck of a steamship—all serve as watchtowers or observation posts for this scientist who puts down what he sees, accurately and entertainingly.

An appendix of scientific and common names clarifies the subject matter for the nonscientific reader.

I read *Edge of the Jungle* when it first came out, and I have read with renewed pleasure the new edition.

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A MATTER OF BIOLOGY

This is Race. Earl Count, Ed. xxviii + 747 pp. $7.50. Henry Schuman. New York.

IN THE Introduction to his book, Professor Count strikes the keynote of the volume when he says that "race is a matter of biology, and nothing else." Actually, he might well have said "morphology;" for all other aspects of biology—physiology, especially—are either omitted or only sparingly mentioned. It is principally the externalities of race, the outer hallmarks, as it were, that are presented. We learn how people look different far more than what the differences mean in terms of functional import; social interpretation is not attempted (and wisely so!). The behavioral patterns of peoples—so-called racial psychology—are mentioned merely to announce that they will not be considered.

The foregoing delimitations are not negative—they are positive, for they permit concentration upon the purpose of the book, i.e., to deal with "the scientific concept of the races of man, not a discussion of attitudes on race." In sixty articles, originally written in nine different European languages by authors of twelve different nationalities, we learn that the concept of race is entrenched in the thinking of all peoples, and has been, for centuries. Race, it seems, is here to stay!

The book is divided into four historical periods: the eighteenth century, to the fall of Napoleon; the earlier nineteenth century, to Darwin; from Darwin to the first world war; the twentieth century since the first world war. In reality, however, the period division is ideological more than it is historical.

The eighteenth century marks the first real documentation of the concept of race. This was the period of the reign of Reason; the search for fact, and its subsequent categorization, were based on the theme of consistency. Hence classification was the order of the day, with man no exception. Systems of races were neat little packages of uniformity, based almost solely on observation alone. New peoples, as they were discovered, were fitted into precise classificatory pigeonholes. Monogenists and polygenists clashed right merrily, with environmentalists hovering on the fringe. The great names of this period are Buffon, Kant, Blumenbach, Linnaeus, Huxley, de Quatrefages, Broca.

After the French Revolution came a challenge of philosophy and speculation. Conjecture gave way to measurement, surmise to experimentation. Ideas in science were not tenable unless they could be tested. So, evolutionary concepts made bold to assail the citadels of dogma: the works of Lyell, Darwin, Huxley, and Haeckel were the battering-rams. It was inevitable that man now be classified not alone for his own sake, but into a much broader biological framework. He became one with the organic world.

From here on the pace accelerates: Cuvier developed paleontology and comparative anatomy; the science of linguistics abetted classificatory schemes; Quetelet expanded biometric methods; Retzins introduced the famed cephalic (cranial) index; Broca formalized physical anthropology, the "science of man;" Mendel, de Vries, Correns, and Tschermak expanded genetics; serology and constitutionalism now emerge as useful methods of classification. In short, the concept of race has progressed from mere philosophical ordering to a reasonably accurate scientific formulation.

This, in essence, is what you will learn from *This is Race.* Strictly speaking, the title might well have been "These are Race," for the several contributions are woven together only loosely by the strands of the summarizing Introduction. For the lay reader and for the beginner in anthropology there might well have been a few evaluatory remarks, for—and it is inevitable, of course—not all the articles are at the same high level of scientific value, even allowing for the time-sequence factor. In a section of Notes at the end of the volume Professor Count identifies the source of each article, and gives a brief sketch of the salient contributions of the author. This helps a bit in deciding the merits of a given reference.

In a compendium the biggest job is exclusion rather than inclusion. On the whole the author has done an excellent job. It is easy, for example, for the reviewer to say that he misses Gates on the five species of man, Ashley Montagu's statement of race as a "fallacy," Redfield's assertion that there are only "socially supposed" races, references on genetics by men like Dobzhansky, and a source-quote on constitutionalism and race by Sheldon rather than by one of his associates, and so on. It is no problem to second-guess another man's work!

In a day when clarity of thinking, and an understanding sympathy of people, one for the other, may promote the chances of peace, *This is Race* is a needed and important book. I'd like to see the politicos the world over thumb its pages and absorb its many ideas, from peoples the world over.

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CORRESPONDENCE

PYGMIES, BUSHMEN, AND THE MEANING OF TRAVEL

MAY I comment, in one and the same short note, on two articles which appeared in the March issue of this journal? In the article "Further African Studies," by Wendell Phillips, it is stated about Pygmies: "The information collected on their way of life added supporting evidence to the view that some sort of relationship exists between the Pygmies of the Ituri and the Bushmen of the Kalahari Desert. It is possible that these two peoples are derived from one racial stock, which at a certain time must have populated the greater part of the African continent."

Many years ago I determined the distribution of the blood groups O, A, B, and AB amongst Bantu, Bushmen, and Hottentots. (*S. Afr. Med. J.*, 1932, Jan. 23.) My figures for Bushmen and the figures of Yadin (XVI Cong. Int. Anthrop., Bruxelles 113, 1935) for Pygmies are given in the following table:

AUTHOR	RACE	NUMBER EXAMINED	BLOOD GROUPS IN PERCENTAGES			
			O	A	B	AB
Jadin	Pygmies	1,032	30.6	30.3	29.1	10.0
Pijper	Bushmen	548	57.1	29.5	6.8	6.6

The distribution could hardly be more divergent, and so a relationship is practically excluded. I also noticed that the purer the Bushmen, the less B was found, which suggests that the Bushmen originally were a race possessing O and A blood groups only.

I bring up these old facts not in any carping spirit, but to prevent this misconception of close relationship between Bushmen and Pygmies, which I thought eradicated, coming to life again. The whole story, however, also gives point to what Alan Gregg wrote in his witty and delightful article on "Travel and Its Meaning." His ideas should be taken to heart by any scientist setting out to visit a foreign laboratory. I refer in particular to page 153, especially the middle part of the first column, but not leaving out the top part of that column! If the members of the expedition whose work Wendell Phillips in such short compass sums up so adequately and admirably had followed the suggestions on visiting laid down by Alan Gregg, this misconception referred to by me would have been cleared up on the spot, in Pretoria.

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HISTORY OF MAGNETISM

THE article on Gilbert and the magnet by Rufus Suter in your April issue is an engaging one, but in the interest of historical accuracy I should like to enter an objection to the author's giving so much credit to Gilbert as an originator of the knowledge displayed in Gilbert's book *De Magnete*. It's a great compendium of the magnetic knowledge of the time all right, but most of the discoveries contained in it were known long before Gilbert, and many of them already had been published.

Take, for example, the making of a lodestone into a *terrella*, a little earth, with meridians traced between the poles as on the earth itself. The author calls this an "epoch-making contribution," a part of Gilbert's procedure in recognizing the earth as a great magnet. Actually, this *terrella* construction was described as early as the thirteenth century, in a letter of Petrus Peregrinus.

Next take what Mr. Suter, along with others, has regarded as perhaps Gilbert's greatest contribution, namely, the discovery that the magnetism which controls the compass originates on the earth and that the earth is therefore a great magnet. Peregrinus himself likens his little earth magnet to the big earth and speaks of the poles. However, the recognition is clinched in the literature by the famous cartographer Mercator, in a letter he wrote in 1546, in which he said of the magnetic pole: "On the Earth, therefore, which remains fixed, this point is to be sought." He here recognized that if the magnetic pole were outside the earth then the deviation of the compass would shift diurnally as the earth rotates.

These publications and a number of others of the early tracts on magnetism appeared in 1898 in *Rara Magnetica*, by G. Hellmann

Much of this pre-Gilbert history of magnetism is now available in plain "American English," thanks to a scholarly series of articles printed during the war in the periodical *Terrestrial Magnetism and Atmospheric Electricity*, published by the Department of Terrestrial Magnetism, Carnegie Institution of Washington. There were two series, one by A. Crichton Mitchell entitled "Chapters in the History of Terrestrial Magnetism," running in these issues: 1932, June; 1937, September; 1939, March. A second series is by H. D. Harradon, comprising largely translations from Hellmann's *Rara Magnetica*, under the caption "Some Early Contributions to the History of Geomagnetism," which ran in these numbers: 1943, March, June, September, December; 1944, September; 1945, March.

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